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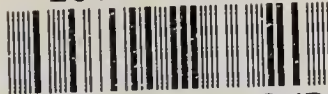
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MINEAL AND MINERAL
ELEMENTS

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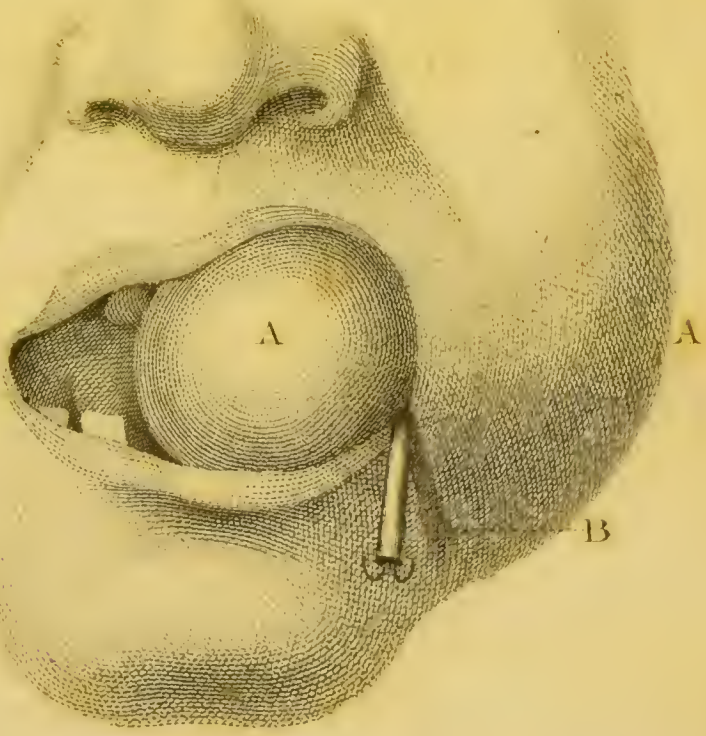


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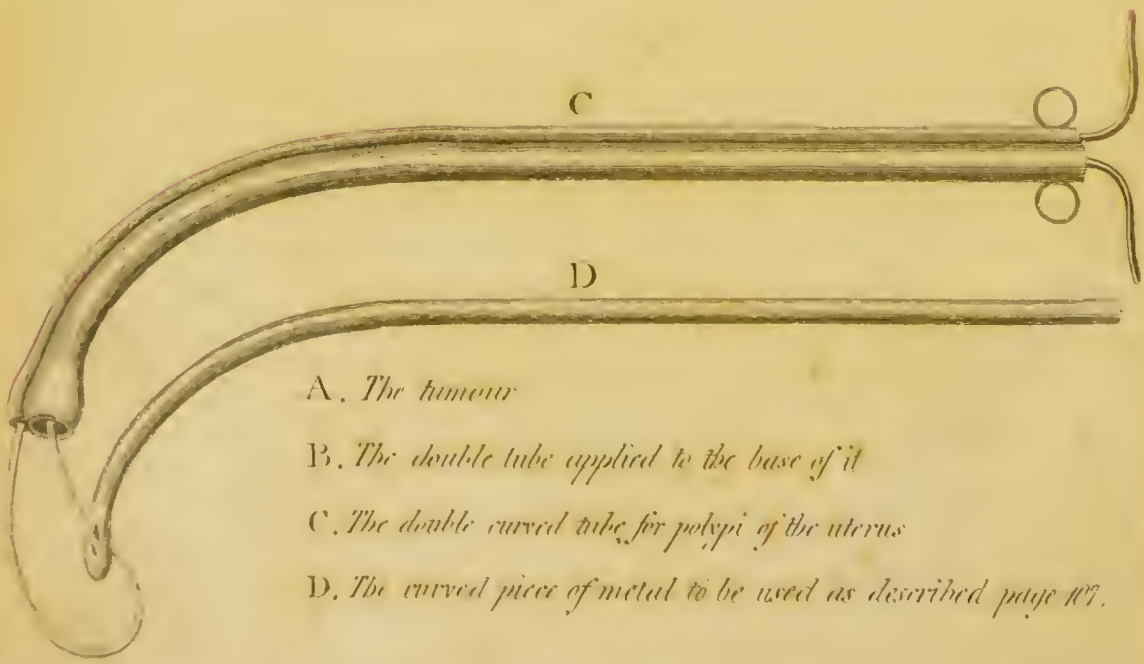
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H. F. Rose Sculp.

Paulsony Sc.



- A. The tumour
- B. The double tube applied to the base of it
- C. The double curved tube for polypi of the uterus
- D. The curved piece of metal to be used as described page 107.

22 5.12.

MEDICAL AND SURGICAL
REMARKS,
INCLUDING
A DESCRIPTION OF A SIMPLE AND EFFECTIVE METHOD
OF
REMOVING POLYPI FROM THE UTERUS, TONSILS
FROM THE THROAT, &c. &c.
LIKEWISE
OBSERVATIONS
ON THE
DIFFERENT MODES OF OPENING THE BLADDER
IN RETENTION OF URINE,
FROM OBSTRUCTIONS IN THE URETHRA
AND PROSTATE GLAND.
AND
A Description of a more safe and effectual
METHOD OF PERFORMING THAT OPERATION,
Illustrated by Cases.

*Aliorum judicio permulta nobis et facienda et non facienda,
et mutanda, et corrigenda sunt.*

CICER. I OFFIC.

BY EDWARD GRAINGER,
MEMBER OF THE ROYAL COLLEGE OF SURGEONS, AND SURGEON
TO THE DISPENSARY AT BIRMINGHAM.

London:
PRINTED FOR LONGMAN, HURST, REES, ORME, AND BROWN,
PATERNOSTER-ROW.

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1815.

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TOMMIL



PREFACE.



THEORY and speculative reasoning so actively and pleasingly engage the mind, that writers have a certain hold on public notice, who largely enter into such disquisitions; but I fear that a mere detail of facts will scarcely engage much attention. As, however, some of the subjects on which I have ventured to give my opinion, are of great importance to human life, I have not thought it safe nor proper to enter into much theoretical discussion upon them, excepting so far as that discussion can be supported and illustrated by cases and practical observations.

To strengthen those cases and observations, and that they may not merely rest on my assertions, I have, on every possible occasion, brought forward as much evidence as can be done with propriety, by mentioning the names of gentlemen who have been present at the respective operations, or by such other proof as the nature of the subject admits of. This is, I consider, particularly necessary in a person, who for the first time, appears before the public as a writer.

On the method of dividing the membranous part of the urethra and prostate gland in certain obstructions to the discharge of the urine from disease in either of those parts, I have been compelled

to examine the opinions of some of those writers who have recommended other modes of operating. I have respectfully submitted what I have observed, and what I have done; and the method which I have recommended will be adopted, or rejected, by the profession, as it may be found on trial to deserve.

That part of the work which contains an account of removing tumours from some of the accessible cavities of the body, will, I trust be of service, by simplifying the method, and therefore rendering more easy the removal of those kind of tumours.

The opinions of two celebrated men on fractures of the skull, which have been widely diffused, and which have been much acted on by the younger part of the profession, have not met with the complete acquiescence of practical men. It has been supposed, that restricting operations so much as they have recommended in their works, does frequently lead to very dangerous conclusions.

This is a matter which deserves discussion, and I hope it will not be long before the attention of surgeons is directed to it, as it can scarcely be safely settled by the opinion of two gentlemen only, however great their abilities and opportunities may be.

As this work may possibly be seen by some of the surgeons of the manufacturing and great mining districts of this neighbourhood, and of Shropshire, in both of which I am somewhat known, I hope it will particularly direct the attention of those gentlemen to this subject, as they must necessarily witness such accidents very frequently.

From the number of accidents of this kind that are continually before their eyes, a mass of evidence may in a short time arise, if they think proper to record it, that will considerably tend to refute or confirm these opinions.

I have ventured to relate some individual cases. After what Mr. Abernethy has said on this subject, it is not very encouraging to do so. Mr. Abernethy says, when relating some cases, that he believes it almost unnecessary to do so, as he thinks few people pay attention to them or remember them.

If he who has brought before the consideration of surgeons one of the most important operations that ever was submitted to them, viz. tying the external iliac artery for aneurism, an operation, the description of which, were all his other able and ingenious works forgotten, would alone transmit his name to posterity as one of the first surgeons of the age. If Mr. Abernethy has to complain of this want of attention, certainly no other surgeon can hope or expect it.

But with great deference to him, I am disposed to think the relation of cases does frequently impress on the minds of medical men much useful detail. Whether I have been fortunate enough to select any which may be serviceable, I must leave to my readers to determine.

It may be proper to slightly notice the circumstance of my having published medical and surgical remarks in one work. On the propriety of combining the practice of medicine and surgery, I do not think proper to give any opinion. It will be sufficient

for me to say, that I found it the established practice in this place when I came to reside here, that it had been so among the most eminent surgeons for many years before, and so it continues.

In respectfully submitting my opinions to the notice of medical men, I have, as much as possible, guarded myself against that delusion which self complacency may produce; and therefore have formed no conclusions but what seem to me to be deduced from facts.

If, however, future experience, or the well founded statements of others, shall prove that I have drawn false conclusions, I shall most readily proclaim my error.

Some of the subjects may, perhaps, be resumed hereafter, if any facts occur which tend to strengthen or refute my opinions.

E. G.

Birmingham, May 1, 1815.

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ERRATA.

For *prostrate*, read *prostate*,*

Page 7, line 25, for *levater*, read *levator*.

157, — 31, for *crista*, read *spinous process*.

Add page 53, line 3, after *prostate*, “ as I found in introducing an elastic catheter, immediately on the silver catheter being withdrawn.”

* It may be necessary to state, that the word *prostrate*, has been printed throughout this work, instead of *prostate*, from this circumstance. The author's time being very much occupied when this work was printing, he only examined *one* proof sheet; and after carefully correcting it, he depended on the conductor of the press, who is a very intelligent person, to correct any trivial error that might have escaped his notice.

The conductor of the press, on consulting his dictionary, and finding no such word as *prostate*, concluded that the letter *r* had been omitted by mistake, and therefore inserted it.

Some few copies of the work got into circulation before the error was perceived.

MEDICAL AND SURGICAL REMARKS.

On Retention of Urine from Obstructions in the Urethra or Prostrate Gland.

THIS disease has engaged the attention of medical men from the earliest ages, as unfortunately it is by no means a rare occurrence. Strictures in the urethra, before the use of bougies was known, must frequently have produced it; and long after their use, it must often have arisen from the same cause, until the important discovery of Mr. I. Hunter's, of applying caustic substances to remove them, was made public.

The passage of the urine through the urethra, is frequently impeded by stones blocking up that passage. Blows on the perinæum, or other injuries on those parts, are often the cause of this obstruction. Disease, or enlargement of the prostrate gland, likewise frequently produce it.

From whatever cause it arises, if the catheter cannot be introduced, it becomes the duty of the surgeon to open the bladder, to obviate the dreadful consequences of inflammation and mortification, and at last, the bursting of the bladder at the mortified part. This terrible event has taken place.

and more than once from the ignorance or timidity of the medical attendant, in cases that I have been called to, after the urine had been poured into the abdomen. It is almost needless to say, that death inevitably follows that event, attended with lingering and excruciating torture.

To prevent this miserable occurrence, several operations have been proposed; and I know not any operation in surgery, that demands more serious attention and consideration. From the nature of the case, it seldom happens that much time is given for deliberation; and on the immediate decision of the surgeon, frequently depends the fate of the patient. It becomes therefore the duty of us all to consider, before the exigent moment calls upon us to act, in what manner we are to perform this difficult and important operation.

The different methods which have been recommended of opening the bladder, are, 1st, puncturing or cutting into it above the pubis; 2dly, puncturing it through the rectum; 3dly, puncturing it through the perinæum; and 4thly, either making an incision through the integuments of the perinæum and the muscles, and then puncturing the bladder behind the prostate gland; or making an incision as in lithotomy, and then passing a gorget through the prostate.

I purposely avoid the pedantry of quoting a variety of authors who have written on this subject; such a display of literature and extensive reading is very easily exhibited, without much expence, either of time or of intellect; and as it

appears to me quite useless, I shall confine myself to observations on the operations which have been recommended by modern surgeons, particularly Mr. Benjamin Bell, Mr. Abernethy, and Mr. John Bell. It may be however proper to observe, that Heister has mentioned these different operations, and has given the opinions of many surgeons of his time respecting them

Heister condemns the method of cutting into the bladder through the prostrate gland, as he says, “thereby the neck of the bladder and urethra are generally cut through without any necessity, whereby the inflammation becomes more violent, and at the same time the seminal outlets in the prostrate are very much injured.”

“It is therefore, he says, a safer and more commodious method in my opinion, if the incision is made in the same part of the perinæum, and with the same instruments, as are customary in the apparatus minor, or in the lateral operation, cutting into the bladder without injuring its neck; after which a silver tube may be introduced, and the urine be discharged as before; by which means the neck of the bladder and urethra are preserved.

There is still, he says, “a third method which seems to be preferable to either of the preceding, which consists in perforating the perinæum and bladder in the same part, but with a trocar instead of the knife, the figure of which may be seen in tab. 24, fig. 1; the trocar being passed into the bladder, its triangular bodkin is then immediately extracted, while its canula remains in the wound,

and gives a freer passage to the urine in the bladder ; which operation is not only more easy and expeditious, but the wound itself will also heal much sooner, and with less trouble to the patient. Nor is it improper in this case to introduce one or two of the fingers into the patient's anus, as is usual in cutting for the stone ; by which means the instrument may be more exactly guided into the bladder, without doing any injury to the rectum," &c. &c.

Mr. Benjamin Bell disapproves of cutting through the perinæum and prostate gland.

He does not recommend puncturing above the pubis ; and absolutely condemns puncturing per anum. " Indeed," he says, " it scarcely deserves to be noticed, as no advantage can possibly occur from it that may not with more certainty be obtained from perforating from the perinæum ; and it is attended with this very material inconvenience, that by passing the instrument in at the back part of the bladder much risk must be incurred of wounding either the ureters, vasa deferentia, or vesiculæ seminales ; while, at the same time, a passage will be formed by which the fæces may find access to the cavity of the bladder, and by which much misery may be induced."

He recommends puncturing through the perinæum. He states, " the patient being properly secured, that an incision should be made of about an inch and a half in length, beginning at the commencement of the membranous part of the urethra, and continuing it towards the anus, in a line paral-

fel to, but at least half an inch from, the raphe perinæi. In this manner the skin and cellular membrane ought to be freely divided, which puts it in the power of the operator, not only to introduce the trocar with more ease, but to avoid the urethra with much more certainty than he otherwise could do.

This being done, as the bladder is always much distended when this operation is necessary, it will be very easily distinguished by pressure at the bottom of the wound ; but whether it should be felt by the finger or not, there need be no hesitation in pushing in the trocar a little above and to the left side of the prostrate gland, which, if the parts have been freely divided, may be always discovered, and if the point of the instrument be directed a very little upwards, there can be no danger of wounding either the ureters, or vasa deferentia, which some have been afraid of in this operation ; and at the same time there must be an absolute certainty, if the trocar is carried deep enough, of its reaching the bladder."

Mr. Abernethy decisively recommends puncturing the bladder above the pubis. He seems to have been induced to prefer this mode of operating, from having been deterred, as he says, " from puncturing per anum, in cases of stricture, where false passages have been made, and the prostrate has been sound, the perception of the bladder from the rectum has been so indistinct that I have been deterred from puncturing it ; and in one case I made a division in the perinæum, and having

passed my finger beneath the arch of the os pubis a considerable way, I could obtain no distinct perception of the bladder as would authorise me to push in a trocar. But I punctured above the pubis, and drew off a considerable quantity of water. I have therefore been led to conclude, that, in some distended bladders, there is a kind of recession of them from the perinæum, and that when they become distended they ascend proportionally higher into the abdomen."

Mr. Abernethy goes on to say, " Sometimes I have been in doubt if there was much urine in the bladder, and this circumstance has deterred me from puncturing (above the pubis) except in that situation in which I could possess an assurance that I felt the bladder, and could puncture that viscus; and these doubts caused me, in some instances, to puncture the bladder with the lancet; and in some cases I have not left any canula in the bladder, in consequence of the escape of the urine preventing me from readily finding the opening which I had made.

" Several of the patients died, but in every instance the operation relieved their sufferings, and I have never seen any effusion into the cellular membrane, or any other bad consequence result from the operation; nor do I think it likely to happen if it be rightly performed. The death of the patients was fairly to be imputed to the delay of the operation, or to the degree of the disease that existed in the urinary organs."

Mr. Abernethy then gives, as one instance of

recovery among others, the case of a gentleman between 60 and 70 years of age, where the bladder was punctured above the pubis for a retention of urine from a diseased prostrate gland. No effusion of urine took place into the cellular membrane, nor does Mr. Abernethy think it likely to happen, either, as he says, "from reasoning or observation, whilst there is a free external opening." He observes, "that the urine issued through an elastic catheter which was left in the bladder, and likewise by the side of it. In three weeks the urine came through the urethra; and in six weeks the patient was as well as before the retention of urine took place."

Mr. John Bell prefers the puncturing through the rectum. He says, in a short history of the origin of this operation, which he states was suggested by mere accident, that a person "did actually strike the trocar into the bladder through the walls of the rectum, and draw off the urine very easily, and had the happiness to find that the canula could be retained, and that when, by accident, it slipped out, the perforation still continued open, so as to permit the urine to pass, and yet the contraction of the levator ani muscle shut the opening like a sphincter, so that the urine never flowed, except when the bladder was sufficiently full to excite desire, and when the natural course of the urine was restored, the opening betwixt the bladder and the rectum spontaneously healed."

Mr. J. Bell goes on to state, "that with such

advantages, with no important parts endangered in the operation, this method is unfortunately limited! it was once practised very happily in a child, whose urine was obstructed by a stone sticking near the glans penis; it has been practised also with the best effects in young men, who have had the urethra burst, by falls upon the perinæum; but it is preferred chiefly where the obstruction is temporary, and where we have reason to hope that the natural course of the urine will soon be restored; but in the most distressing of all cases, the disease of the prostrate, this method, I fear, cannot be used."

Mr. J. Bell then goes on to say, that in so desperate a case, "the surgeon feels that he could push the catheter on to the bladder! not perhaps in the regular canal, but perhaps it might tear through the membranous part of the urethra, and plunging through the substance of the diseased prostrate." We have no authority, he says, for this, farther than in a desperate case, a desperate remedy must be resorted to.

Mr. J. Bell says, that Mr. Dease avowed to him, that he had been in the habit of driving his catheter right onward to the bladder when art or cunning failed. He assured Mr. Bell of his success, and was not deterred by any degree of difficulty. Mr. Bell very properly hints, for he does not distinctly recommend, that this may be had recourse to by men of judgment and skill in very desperate cases; and after pointing out the danger of using a trocar-pointed-catheter, concludes by saying,

“ the catheter is, I am persuaded, sufficient, if skilfully used, to make it's way through almost any obstacle.”

Mr. J. Bell reprobates in very severe language, the operation of cutting into the perinæum, and says that “ the latter operation, viz. the cutting into the perinæum, which had been founded on driving the gorget through the neck of the bladder, had fallen into utter contempt when Sharpe wrote his critical analysis, and how any thing so nearly resembling this cruelty, as the modern operations, by incisions in the perinæum, could be thought of, is to me a matter of surprise.”

Sharpe says, “ besides these methods of drawing off the urine when under suppression, they have also made way for the reception of a canula, by cutting open all the urethra from that part of the perinæum where cutting is performed by the greater apparatus, and continuing the incision through the neck of the bladder; this they have done by the help of a grooved canula, and cut upon the groove when the incision was made by a gorget, and by that means a silver canula was introduced, round which they twisted some fine rag, that it may lie easier in the wound. “ This,” says Mr. J. Bell, “ is, indeed, making surgical work of it.”

Mr. J. Bell afterwards goes on to say, “ let us reflect on this operation and its consequences. When the catheter is obstructed, and can by no means be forced into the bladder, what is the difficulty? some stricture amounting to almost an

obliteration of the membranous part of the urethra, or more frequently an induration of the prostrate gland. Where is the catheter arrested? always near the point of the prostrate gland; it stops just where the point of the catheter passes the finger laid on the perinæum, and at that place where it is felt upon introducing the same finger into the rectum. If the catheter could be thrust through this obstruction, and driven into the bladder, even at the expence of some violence, and much blood, would such rudeness be fatal? by no means; such an operation would, on the contrary, give present relief.

“ If either by this pushing on the catheter with some laceration of parts within, or if, by a small incision in the perinæum, the course of the urethra could be recovered, and a probe passed along it, or a common grooved director, or any such instrument introduced, would such injuries be at all equal to those I have enumerated? or would the danger of inflammation, where the neck and natural opening of the bladder were thus forced, be at all comparable with these dangers, which must accompany a wound of its body, whether from above the pubis, or from behind the prostrate gland? surely not. If surgeons would condescend to make themselves masters of this piece of anatomy; if, having passed the male catheter as far as it would go, they would then, by a small incision, seek out the obstructed part of the urethra, they would be able either to help on the catheter with the probe, or to dilate the urethra, or to force

the prostrate gland, by passing a female catheter straight on from the point where the male catheter stops; and a wound so bloodless, so far from the pelvis and abdomen, so nearly resembling the thrusting on of the catheter, affecting merely the neck of the bladder, might be resorted to early, and many precious lives be saved.

If there be a method which may be put in competition with this, it is that of puncturing the bladder from the rectum. I am not indeed ignorant that this way of reaching the bladder lies under the formal protest of some writers, but they are of those who have written about surgery by conjecture, whose authority stands on no other ground than reasoning; and what reasoning can discover any thing superior in the common and rude operation of making incisions in the perinæum, when compared with this of puncturing from the rectum."

It has been absolutely necessary to detail these different plans of operating, that the reader may have the whole in his recollection, while I am discussing this important point; to what mode of operating shall we have recourse, when called upon to assist a human being in the afflicting situation of not being able to discharge the urine from the bladder.

In giving my opinion on this important question, I most seriously feel the great responsibility which I have imposed on myself. I feel how much I expose myself to the imputation of arrogance and presumption, that I, unknown as an

author, should impugn the opinions of enlightened men, who, from their situations and writings, have been long looked up to as established authorities.

And indeed presumptuous and arrogant I should consider myself, were I to hazard opinions in opposition to these distinguished gentlemen, if those opinions were supported only by theoretic reasoning, and not by the strong and incontrovertible evidence of facts.

But, supported by facts; with cases to refer to, in which I have operated myself; and likewise, when other surgeons have, under my directions, performed, with success, the operation which I am about to describe; I should consider myself as having disgracefully shrunk from my duty, were I to suffer myself to be deterred from publishing my opinions.

I pay great deference to respectable authorities, but I cannot allow facts, which contradict them, to pass without observation.

Having thus, as I trust, sufficiently apologised, I shall proceed to observe on the opinions of those gentlemen with perfect freedom, and I will take care that it be done with decorum.

It perhaps may not be necessary to review the opinions of Heister, as his system of surgery has been in some degree superseded by later writers; and as the different methods of operating mentioned by him, will necessarily be noticed when I am observing on the methods recommended by the other gentlemen whose names I have men-

tioned. This will be the more unnecessary, as although so many years have elapsed since he published his system of surgery, he distinctly mentions every method of operating in this terrible complaint, which has been mentioned by later writers.

In remarking on the writings of Mr. Benjamin Bell, it is impossible not to be struck with the hesitation and want of decision that pervades his whole work. He writes like a theorist. He wants that firmness of description that carries his reader into the very scene of operation with him, and convinces every one, that he describes what he has himself done, or has seen others do.

The operation which he recommends to be performed in retention of urine, appears to me to be the very worst that can be selected. His reasons for condemning the puncture per anum are very futile. What surgeon that deserves the name of surgeon, would be afraid of wounding the ureters, or vasa deferentia, or vesiculæ seminales? His fears of the fæces being forced into the bladder are quite groundless. Other reasons there are, and much more likely to occur from the puncturing through the rectum, and which I shall state hereafter, for condemning that operation. His manner of performing the operation which he recommends is, in my opinion, not good. If it is determined to puncture the bladder from the perinæum beyond the prostrate gland, there can be no reason for making an incision an inch and a half long first of all, and, as he says, half

an inch from the raphe. The bladder being immensely distended in all cases which require any operation, it will be no difficult matter to convey the point of a long trocar into its cavity, without any other guide than a knowledge of the anatomy of the parts; but if the surgeon should have any doubt on the possibility of doing this, his finger being introduced into the rectum would be a sure guide to him. But it is not so much to the method of performing this operation that I object. I object to the operation itself. It is radically bad. It has all the inconvenience and difficulty which attend the other operations, and more danger.

Exclusive of the secondary consequences which must be expected to be produced by such a long narrow opening having been made among cellular membrane, the primary, the immediate danger must be very great. Granting that the point of the trocar be safely conveyed into the body of the bladder, beyond, and to the left of the prostate gland, which I have not the least doubt can be done; still we must recollect how irritable the bladder is, and how liable inflammation, when once excited in it, is to run to great extent; and how readily this may extend to the peritoneum that covers the back of it; and when once diseased action is excited in that irritable membrane, we know how difficult it is to control, or stop it. All these consequences may very probably take place in a few days after the operation. But if this immediate danger is got over; if there be no inflammation of the bladder or peritoneum, what

are we next to expect? After the incision has been made in the perinæum, the trocar is then to be pushed through the levator ani, and a long extent of cellular membrane, into the bladder, beyond the prostrate.

For some days a canula or flexible catheter is to be kept, if possible, in this opening.

Sooner or later this catheter must be removed. Then begins a miserable scene. The urine cannot be carried away through this long narrow hole. The prostrate becomes inflamed, and therefore no water can pass through it. The urine is therefore diffused through the cellular membrane in all directions; between the bladder and rectum, and about the perinæum; and if fatal inflammation does not come on, perhaps, after weeks and months of suffering, new operations are to be performed under every possible disadvantage, on a person worn down by profuse discharges, and pain, and anxiety, to attempt to remedy the miserable blunders of ignorance.

This is not what is merely possible, or likely to take place; I have seen such cases, and have had enough to do for months, attempting to remedy what ought never to have been produced.

These were the reasons, and others of a similar nature, that induced Cheselden, and the great surgeons of his time, to leave off cutting into the bladder beyond the prostrate, in lithotomy; and how it entered into the minds of any reflecting men, to propose such an operation as this, when they were acquainted with the danger and incon-

venience which attended such deep and oblique wounds whenever made by a knife, is most surprising.

This I am quite certain of, that no person will perform this operation a second time.

Mr. Abernethy, as I have observed, decisively prefers puncturing the bladder above the pubis.

Whatever this gentleman writes, demands and deserves the greatest attention.

His great opportunities for acquiring surgical skill, his attentive researches, his profound anatomical knowledge, and his many admirable works, have placed him among those great men who have done so much for our profession.

Impressed as I am with that respect which all these considerations naturally give rise to, it is with pain, that I am compelled to differ from him on this important subject.

The first objection which I make to opening the bladder above the pubis, is this very important one; that by this mode of operating, the body of the bladder itself is wounded.

The danger of inflammation in that viscus, and of peritoneal inflammation also, must be very great in the first instance; as I have endeavoured to shew before, when speaking of the operations that Mr. B. Bell has recommended. Exclusive of this, the difficulty of keeping a canula in the bladder, after it has fallen down some inches, which it inevitably must do, after the urine has been discharged; the consequent effusion of water into the cellular membrane, which I should sup-

pose would inevitably take place; the inflammation which would be thus produced, and which must extend, rapidly and extensively, to the bladder and peritonæum, would deter me from ever attempting, thus, to evacuate the contents of the bladder.

In fact, it would appear to me, that if inflammation should be brought on by effusion of urine into the cellular membrane, it must be more dangerous in this situation, than the inflammation which might be brought on from the same cause, in those parts which are operated on according to Mr. B. Bell's plan.

Practically, I know nothing of these consequences; as I have never seen the operation performed; but I have heard enough on the subject, from surgeons who have punctured above the pubis; and their reports have deterred me from operating on that part.

Indeed, Mr. Abernethy himself tells us, with great candour, that several of his patients died; but he thinks, that death could not fairly be attributed to the operation.

It would have been more satisfactory, if Mr. Abernethy had been pleased to have given a short history of some of those cases. Then it would have been more clearly shown how these persons did die; and from the symptoms, a fairer inference could have been drawn as to the effect of the operation, and whether death was attributable to it, or to the disease; whether, from inflammation of the bladder, brought on from the opera-

tion having been too long deferred; or from disease, the consequence of the mode of operating.

In the very short and obscure way that Mr. Abernethy has thought proper to mention the deaths of the persons whom he had operated upon, no clue is given, satisfactorily to explain the cause of their deaths.

I think Mr. Abernethy does not discuss this important subject with all his usual ability. He evidently had not considered it very minutely, when he wrote; nor balanced the advantages and disadvantages, which should be considered in the different methods of operating in this dreadful complaint.

The only case which he relates, is of a gentleman who recovered in about six weeks; and he states, in that case there was no effusion of urine into the cellular substance; and he says, "from reasoning he did not expect there would." It is strange what very different conclusions may be drawn from the same premises. I should have thought, considering the situation of the puncture, the depth that the bladder recedes from the integuments, &c. that effusion was almost inevitable. Mr. Abernethy does not tell us what he did to cure the cause of this obstruction, the enlarged prostate. Puncturing above the pubis could have no effect in removing that enlargement.

I do not know whether I am to infer from Mr. Abernethy's writing, that this was the only case that terminated favourably. He does not men-

tion any other.—If only one case did succeed, I think it is going too far, to draw general conclusions from one solitary case in favor of puncturing above the pubis.

I conjecture that Mr. Abernethy himself is not quite satisfied with this mode of operating; as he tells us, “that in one instance, he made a division in the perinæum, but that on passing his finger beneath the arch of the pubis a considerable way, he could obtain no such distinct perceptions of the bladder as could authorise him to push in a trocar;” and therefore, he, after having cut into the perinæum, finished this operation by puncturing above the pubis.

As to what he afterwards states about the receding of the bladder in consequence of distension, and of its ascending proportionally in the pelvis, it is not founded in fact; it is an erroneous opinion. The upper part of the bladder of course ascends; but the bottom of it, the prostrate and that part of the bladder, certainly does not ascend. On this subject I can speak positively. I know that this assertion is wrong, from repeated examinations of the parts in every state of disease.

We have here, I think, an instance, how a gentleman of the greatest practical skill may be misled, by depending too much on mere authority.

If Mr. Abernethy had depended on his own excellent anatomical knowledge, and not have suffered himself to have been misled by assertions; if, when he had cut into the perinæum, he had

dissected a little farther, he would have found the prostrate gland in its proper place; and if he had then passed his knife through it, and simply divided it, instead of seeking for the bladder bulging behind it, which I am afraid he intended to puncture in the manner recommended by Mr. Benjamin Bell, he would have found, that he had not only relieved his patient, but in all probability cured him; and this would have been effected by performing one operation instead of two.

Mr. John Bell recommends puncturing the bladder through the rectum.

Perhaps no two men ever differed more in their style of writing, than this gentleman and his namesake, Mr. Benjamin Bell.

In Mr. John Bell's writing, all is luminous and decisive. His very exuberance, which the critics have complained of, is pleasing; it is the overflowing of a mind rich in science, and profoundly skilled in the history and knowledge of our profession. His descriptions are so vivid, his conclusions are generally so just, and his language is so seducing, that he carries his reader onwards by his vivacity, and compels attention. He rouses the indolent, and delights even the dull and phlegmatic.

In treating on the important question of which is the best manner of operating in this afflicting disease, he does not, however, in my humble opinion, discover his usual discrimination and acumen.

He wavers and is not decisive. I think he was not himself satisfied with what he has written.

It is absolutely necessary, to understand his meaning, carefully to read over what he has written more than once; as he confesses that his favourite method of puncturing through the rectum, does not hold out a prospect of success in those deplorable cases, where, as he says, the membranous part of the urethra is almost obliterated, or where the prostrate gland is diseased or enlarged.

He, in one place, in very strong language, reprobates the practice of cutting into the perinæum and dividing the prostrate with a gorget, and states, that in Sharpe's time it had fallen into discredit. Sharpe says, "they have also made way for the reception of a canula, by cutting open all the urethra from that part of the perinæum where cutting is performed by the greater apparatus, and continuing the incision through the neck of the bladder; this they have done by the help of a grooved staff, when it was practicable, and, where strictures of the urethra prevented the introduction of the staff, they have either cut according to the best of their judgment, without any guide, or have pushed in a trocar with a grooved canula, and cut upon the groove; when the incision was made they passed a gorget, and by that means a silver canula, round which they twisted some fine rag, that it might lie easier in the wound."

This, Mr. John Bell calls a most atrocious operation.

If we except what is said about the use of the gorget, it does not appear to me to deserve to be

so stigmatized. It certainly was not necessary to use that instrument. The prostate might have been divided with much greater ease, and, what is of much more importance, with much greater safety. Indeed it could not be necessary to introduce the gorget, where they had introduced, as Sharpe tells us, a trocar with a groove, through (I suppose he means the prostate) into the bladder; as the urine would of course be then discharged; and for what purpose the gorget could have been used afterwards, I cannot conjecture. That part was certainly quite unnecessary. But if that part were omitted, there are cases, I think, where Mr. J. Bell himself would recommend something like that mode of operating.

In Mr. J. Bell's 2d vol. page 285, he says, after having written pretty largely on the different methods of operating, "if having passed the male catheter as far as it would go, they would then, with a small incision, seek out the obstructed part of the urethra, they would be able either to help on the catheter with the probe, or to dilate the urethra, or to force the prostate gland, by passing a female catheter straight on from the point where the male catheter stops, and a wound so bloodless, so far from the pelvis and abdomen, so nearly resembling the thrusting on of the catheter, affecting merely the neck of the bladder, might be had recourse to early, and many precious lives be saved."

There certainly is no great difference in this mode of operating from that which Sharpe de-

scribes, excepting what he says about the gorget. The dissecting beyond the point where the catheter stops, is cutting according to the best of a person's judgment; and the helping on the catheter as far as it would go, and then dilating the urethra, and forcing a female catheter through the prostrate gland; Why, what is the difference between this, and what Sharpe tells us the surgeons of his time did, and which Mr. J. Bell reprobates so severely? Sharpe says, they having reached so far by the aid of their judgment, pushed a grooved trocar through into the bladder.

There is no difference in the principle on which both operations are performed; and what difference there is, is only in terms.

The operation, which Sharpe tells us the surgeons of his time performed, except the use of the gorget, is the best. When the prostrate is got to, a trocar may be passed through it with greater ease than a blunt catheter; and as to the safety, that must be the same; as there can be no danger of the parietes of the bladder being touched by the point of the trocar, as the distended state in which it is, in such cases, would prevent the possibility of it.

What Mr. Bell says about helping on the catheter with a probe, and dilating the urethra, &c. that certainly is true. I apprehend from the manner in which Mr. Bell mentions it, that he has not actually done it; but the suggestion is a good one. It is in some cases practicable, as

I know from experience; and in others, I can positively state, that it is actually impossible.

Consider a case such as Mr. Bell properly describes, where the membranous part of the urethra is almost obliterated; a disease of ten or a dozen years standing, where the miserable patient has never been able to squeeze the urine through it but in drops, or in the smallest possible stream; being sometimes half an hour or a whole hour, as I have often been told by them, in discharging the urine. When obstruction comes on from any exciting cause, how is the catheter to be helped on in such a case, or how is such an urethra to be dilated, where, instead of a canal, an almost impervious cord is found?

I have tried to do this and have been foiled, as others will too often be, if it is attempted.

As to forcing a female catheter through the prostate gland, I think that a very bad practice, after the surgeon has reached so far.

I suppose this recommendation is grounded on what Mr. Bell learnt from Mr. Dease.

I cannot suppose that so candid a man as Mr. Bell is, could be influenced by the decisive reprobation which he had previously given to the method of cutting through the membranous part of the urethra and prostate gland, and that, therefore, he has recommended this rude way of getting into the bladder, by driving a catheter into it.

From mere reasoning it would appear, that, having reached the prostate, and finding that the obstruction was in that gland, the easiest and

safest way of emptying the bladder would be to introduce the knife through it. From the operation of lithotomy, we know very well, that no injury would arise from this division of it. The pushing a blunt instrument, like the catheter, through such a firm body, I know, as I shall hereafter relate, is no easy matter. It must tear or stretch the parts very much; and if inflammation should come on, as come on it most likely will, the termination could not be looked to without great alarm and anxiety.

Mr. Bell, however, recommends this mode of proceeding only in some of those cases where the membranous part of the urethra is almost obliterated, and in some very bad cases of enlarged prostrates. In other obstructions, and in such as are of a temporary nature, he recommends puncturing per anum. He does not go very minutely into the subject; and gives only one case, where he says, it was successfully performed. That was the case of a child, where the urine was obstructed by a stone sticking near the glans penis; and it has been practised, he says, with the best effects in young men, who have had the urethra burst by falls on the perinæum.

Mr. Bell says, that “after having punctured per anum, if the canula should slip out of the bladder, the levator ani will shut up the opening, so that the urine will only be expelled through it when the bladder is so full as to excite the desire of emptying it.”

It would have been very satisfactory if Mr. Bell

had given particular cases ; as in one case that I have seen of this kind, the urine had made its way into the cellular membrane between the anus and the bladder ; and although the obstruction of the urethra, being temporary only, had been sometime removed, the water was only partially expelled through the natural passages ; fistulous openings having been formed in the perinæum and about the anus, from whence urine was continually oozing.

Superficial incisions had been partially made when I saw the case, and the person had been put to great and useless pain. There was great inflammation and excoriation about the parts ; and it appeared to me, that nothing could possibly relieve him but making very deep incisions ; and in one part, an operation similar to that for fistula in ano was required.

I explained to the man what I thought necessary to be done ; but as he had suffered so much, and had been so often cut for the fistula, as he properly enough called it, he had no faith in my predictions. I could not prevail on him to submit, and I saw no more of him. His health was very much impaired ; which I understood had arisen from the pain and torment which he had so long suffered ; and I should suppose, if he did not fall into the hands of better surgeons than those that he had been cut by, as he called it, he must ultimately have sunk under his sufferings.

From having witnessed this miserable case, I the more lament that Mr. J. Bell has not given

cases in detail ; as from the light manner in which he treats this mode of operating, merely saying, "that it was once practised very happily in a child whose urine was obstructed by a stone near the glans penis, and in young men who have had the urethra burst ;" it would lead his readers to suppose that it is a very trifling affair to perforate through the anus ; that no awkward effusions into the cellular membrane ever take place ; that no fistulous openings are ever formed between the bladder ; but that the levator ani acting like a sphincter, the water is only discharged into the rectum when the patient wishes to evacuate the bladder ; and that the original obstruction being removed, the punctured parts unite, and all is well again.

Mr. Bell certainly must know that this is not always the termination ; and I am surprised that he, who so anxiously and laudably labours to give the most careful directions to his pupils, and to guard them against the various difficulties that may occur in operating, and in the after treatment, should, in this important complaint, have failed to point out to them the various untoward circumstances that may possibly happen, from this, apparently, simple mode of operating.

Mr. J. Bell seems peculiarly unfortunate when writing on this subject.

Without affecting minute criticism, it is impossible that I can pass by the case of the child, which Mr. Bell mentions as having had the bladder punctured for an obstruction of the urethra,

which obstruction had been caused, by a stone being lodged *near the glans penis*.

No one can despise more than I do, that hypercritical and cynical disposition that delights in magnifying the trivial errors and mistakes of eminent men. I possess not that kind of pitiful ambition that would wish to establish a character for critical acumen and deep research, by hunting out, with microscopic and malignant eye, the slight lapses and oversights which may escape, in the hurry of composition, from the pen of a great author. And little, indeed, should I feel pleasure in pointing out the unimportant errors of a gentleman who has done so much for our profession as Mr. J. Bell has done. But were I to pass over the very bad practice, which is sanctioned, in the relation of this child's case, it would appear that I was either ignorant, and knew not to what erroneous treatment it would lead; or that I was overawed, and was deterred from noticing it, by the fear of retaliation from the powerful pen of Mr. J. Bell.

Having said thus much, I must observe, that it does not often happen that the obstruction caused by a stone sticking in the urethra is so complete as to render it necessary to proceed to an operation merely to empty the bladder. An operation is very frequently necessary in such cases, where the stone is so large and impacted in the passage, that no dilatation of the urethra would enable it to pass. But certainly, it does appear to me surprising, that such a surgeon as

Mr. J. Bell should have even contemplated such an operation as puncturing through the rectum, to relieve this trifling obstruction. Mr. Bell must indeed consider it as a very unimportant and trivial operation, when he resorts to it on such trifling occasions.

The words, *near the glans penis*, are certainly rather indefinite ; but as Mr. Bell applies them, they appear to me to imply, that the stone was in that part of the urethra which nearly touched the glans penis. In such a case, all that could have been necessary, would have been to cut down upon the stone, and to have removed it with a pair of forceps, if it did not fall out upon the opening being made, which has happened to me. Even if the stone was farther from the glans penis, than I infer that it was, from what Mr. Bell says, still, it could not have been necessary to have proceeded to this very serious operation. If the stone could not have been cut upon, on account of its being fixed in that part of the urethra which is covered by the scrotum, still a slight incision might have been made into the urethra in the perinæum, and thus the urine would have been evacuated. But this is mentioned under the idea that the stone would ultimately pass ; for if the stone was so firmly fixed, so impacted and wedged in, that after all possible dilatation of the urethra there would not have been room enough for it to be propelled by the impetus of the urine, the misfortune is, that after all this operating, either by cutting into the perinæum, or,

as Mr. Bell recommends, by introducing a trocar through the rectum into the bladder! the poor child would have had to undergo another operation, and which, if any were necessary, ought to have been had recourse to at first; viz. to cut through the urethra upon the stone, and to liberate it.

Certainly, this was very bad practice, and deserves to be reprobated in as strong language as any which Mr. Bell so often uses.

What would Mr. Bell have said to any poor unfortunate wight, who, to display his skill in operating, had been tempted to have recorded such surgery as this?

A terrific philippic would have been hurled at the unfortunate blunderer's head, and perhaps at his heart; and in a vigorous well rounded period, he would have been "damned to eternal fame," for his ignorance and stupidity.

The other cases which Mr. Bell mentions, where the puncture per anum has been practised with success, seem to me to have been of that kind which did not require that operation.

When the urethra has been burst by a fall on the perinæum, if the integuments and the urethra are torn, all that could be required, if any thing was required by way of operation, would be to make it a clean incised wound, instead of a ragged torn one; and if inflammation should come on, and by thickening the parts, gorge up the passage, so as to impede the flow of urine, all that could be necessary would be to introduce a

probe or director, if that could be done, and to cut upon them ; or if that was not possible, to cut, as Sharpe says, according to our judgment, so as to open beyond the obstruction.

If the parts are bruised, and the urethra torn and burst, without an external wound of the integuments ; as the urine will, certainly, in such a case, be effused into the cellular membrane, producing great inflammation and obstruction in the first instance ; and afterwards, if those should be by any means removed, fistulous openings ; in this case, puncturing through the anus, or any where else but through the part, can never be required. In such a case, a clear incision through the parts would make an outlet for the urine, till, by proper means, the inflammation could be lowered ; and by the introduction of a flexible catheter, or not, as may be necessary, for I know this is not always necessary, the wound would unite, as is seen after lithotomy, or after operations for fistulous abscess in the perinæum, and the urine would be conveyed through the natural passage.

I am here relating what I practically know can be done ; and I am almost as much surprised at Mr. Bell recommending the puncture per anum in such cases, as in that of the child ; with a piece of stone sticking in the urethra, near the glans penis.

In many other cases of obstruction, Mr. Bell acknowledges and laments that it cannot be had recourse to with any prospect of success.

Having thus taken a view of the methods of operating which have been recommended by modern surgeons, in which, indeed, is included a review of those which have been mentioned by the more ancient writers; and after having fairly submitted to the consideration of my readers the objections which present themselves to my mind, against the different operations that have been practised; I shall take leave to suggest a method of operating, that I think is better calculated to relieve, and in many instances, to permanently cure, those unhappy men who are afflicted with this most dangerous complaint.

In doing this, I shall most carefully abstain from venturing on theory or conjecture. I shall adhere strictly to facts that have come under my own observations. I shall only relate cases that have occurred to myself, and where I have operated; or where other surgeons have operated on my suggestions, and under my directions. Where the lives of men are at stake, I feel how necessary it is to make no rash assertions. I will fairly tell what I know. I will relate what has been done in several instances with complete success, and therefore what may be done again.

The different methods of opening to the bladder in obstructions of the urethra and prostrate gland, became very early in my surgical practice a serious subject of consideration with me. When I was a very young man, I happened to see two cases of this painful complaint. In one of these cases, the wretched man was left to die in the most ex-

cruciating tortures. In the other, the operation of cutting into the perinæum and puncturing the bladder, I believe behind the prostate, was had recourse to. I say, *I believe behind the prostate*, because I could not obtain any accurate account from the operating surgeon; for although he was a steady and bold operator, and had the care of a large hospital, I know that he had no very minute anatomical knowledge, and I rather think he did not know himself, precisely, where he had penetrated.

This man died. Peritoneal inflammation came on, which killed him in a short time. He lived, however, long enough to convince me, that this was not the way of operating which promised to be successful. In the restlessness that came on, it was found impracticable to keep the tube in the opening which had been made; it was continually being displaced; and the urine made its way into the cellular membrane about the rectum. I have not the smallest doubt of what would have been the consequence if he had lived sometime longer.

Several years elapsed without another case having occurred to me. By this time I had seen a good deal of practice, and had frequently performed operations. I was called to a young man who had long laboured under strictures low down in the urethra, and who from hard drinking and great exertion had brought on such an inflammation about the urethra and perinæum, that there was a complete stoppage of the urine.

When I first saw him he had been nearly two days without passing more than a spoonful of water. The catheter was attempted to be passed in vain. He was bled and purged, and placed in warm water, and the catheter was again attempted to be introduced. The bladder was prodigiously distended, and was so palpably felt above the pubis, that I was almost tempted to push a trocar into it at that part. As I knew of the stricture in the urethra, I was afraid, if I did this, that I should only give him temporary relief; and, as I thought, that if nothing more was done, fistulous abscesses would be formed in the perinæum if he outlived the operation, and therefore, that openings in that part would be afterwards required, I determined to open beyond the stricture if possible. I introduced the catheter as far as it would go, and deliberately dissected till I discovered the catheter against the obstruction in the beginning of the membranous part of the urethra. As this canal was fairly exposed, I determined to try to get beyond the stricture; and having succeeded in doing this, the straining, which the operation and the distended state of the bladder caused, produced such fulness, like a large vein, in which the blood has been obstructed by a ligature upon it, that it was evident, and distinctly felt. I made an opening in this part, and the water gushed out in a torrent. I determined to cut through the contracted part; which was cautiously done. A flexible catheter was introduced through the penis, and passed

into the bladder ; where it was kept as constantly as my patient's feelings would permit it ; and in a few weeks this person perfectly recovered.

- This was a fortunate case, but it was a simple one, and very favourable for the operation. It was not made difficult by other obstructions, and various other difficulties. The only awkward circumstance that attended it, was, cutting through parts greatly inflamed. Such favourable cases as this was, do not often occur ; and were such an one to occur to me again, and if I could, from the previous history of the case, or by any other means, ascertain that the obstruction was so limited, as it was in this, I think that I should be able to make my way with the catheter, without all this operating.

But at the time I am speaking of, I knew no better ; and from the situation I was in, I was precluded from the possibility of seeking advice from a better and more experienced surgeon. I had not at that time got rid of an over cautious timidity, which I had been taught in introducing the catheter. It was then the custom to teach young men, that any thing like force was to be completely avoided in the introduction of that instrument ; and I know surgeons even now, for whose talents I have much respect, who very sacredly adhere to these fears, and who would shudder at the idea of using any, but the most gentle pressure. One drop of blood following the withdrawing of the instrument, would make them despair, and give up all as lost.

I however now am not quite so timid as I then was; and although I should not venture to follow the advice of Mr. Dease, as related by Mr. John Bell, I have frequently used very considerable force in dangerous cases of obstructions; and have many times succeeded in pushing the catheter through obstructions, that I do firmly believe would otherwise have rendered it necessary to cut into the bladder to relieve.

I scarcely dare mention this, lest it should tempt any of my young readers to use unnecessary force; for language does not afford us expressions to enable us to convey the idea of the precise degree which may be safely used, and which I think I perfectly well know; but I am deficient in words which can convey it to others.

Perhaps it may be enough to say, that no force ought to be used, but in dangerous cases; and as Mr. Bell well says, in desperate cases, desperate remedies must be had recourse to.

In this case that I am speaking of, I believe that I unnecessarily cut into the urethra; and were such an one to occur now, I should try what the catheter would do, and if that failed, then I should proceed as I have stated.

Be this as it may, this case afforded me a great deal of practical knowledge. I found that it was not impracticable, nor indeed very difficult, to dissect to the prostate, for I touched it with my finger; I discovered, that I could find my way thither without a staff to direct me, and that the hemorrhage was trifling. I reconsidered all that

I knew, and all that I had read on the subject; and after pondering on what had occurred to me, and viewing it in every way, I determined, whenever a case occurred of obstruction in the neck of the bladder, if it could not be removed in any other manner, to cut through the prostrate.

Heister tells us, that this had been practised before his time, and he gives the names of different authors that recommended it. But after discussing the question, he decides against it. Sharpe, as I have before said, condemns it without mercy; but it must be recollected, that the manner of dividing it at that time, was with the gorget, and I think it must have been with the blunt gorget, as, if I recollect right, the cutting gorget had not been introduced into practice at the time that Sharpe wrote. To divide the prostrate with a gorget, or rather to attempt to divide it, must have been a difficult matter, even with a grooved staff. But in an obstructed prostrate, there could be no admission for the staff; and, therefore, how it was to be done, or attempted to be done, I am at a loss to conjecture. With a cutting gorget, it would indeed have been miserable work; but if the gorget was a blunt one, it must have forced the prostrate gland before it, and torn the whole bladder from its connexion with the penis. To divide the prostrate with a blunt gorget, when the grooved staff could be introduced, as in lithotomy, was a dreadful practice, and yet it is surprising how long it continued to be used. I recollect that many sur-

geons continued to use it so late as 1783 and 1784; although the cutting gorget had then been long known.

But to return from this digression. The anathema of Sharpe's against the use of this instrument was deservedly just; but in the violence of his indignation he reprobated the principle of the operation, when his abuse should have been aimed at the manner of effecting it.

Mr. Benjamin Bell likewise decided against it; and it was not taught by the lecturers on surgery, or indeed, if mentioned, it was only to reprobate it as violent and impracticable.

Previous to my having operated in this way, I mentioned the subject to many of my friends; but they all thought it too hazardous. The dread that they entertained, was, the difficulty which they apprehended there must be in reaching the prostrate gland, without the groove of the staff to guide the knife there.

As I was in London soon after, I took that opportunity of minutely re-examining the parts by dissection; and I became more confirmed in what I had learnt from my former operation. It was impossible to doubt about the safety of dividing the prostrate and the membranous part of the urethra; as that was done continually in lithotomy, and in the rudest manner. If they would unite and recover so perfectly as to carry off the urine, after being torn and bruised by the passage of the gorget through them, there could

not be a question about their re-union after being gently divided by a knife.

I determined, therefore, whenever a case occurred, to dissect down to the obstructed part of the urethra, being guided to it by introducing the catheter as far as it would pass; to dissect to the urethra, and to get beyond the obstruction if possible; but if the obstruction should be in the neck of the bladder, to cut through it with a knife; then by the introduction of a flexible catheter to keep the parts open a short time, till the external parts began to unite.

I have tried to open the passage in the way that Mr. John Bell recommends, by introducing a probe before the catheter, &c. and have sometimes succeeded; but this will seldom succeed in old cases, where the passage is almost totally obliterated. This should always be attempted before proceeding farther.

The great advantages of the method of operating which I recommend, are, first, that there is no effusion of water into the cellular membrane; and, consequently, that there can be no danger of fistulous abscesses being formed. The external opening so nearly corresponds with the internal incision, that, a priori, I thought no such events were to be apprehended; and this has been placed beyond all doubt, as none such have occurred in any case that I have seen.

The second is, that the body of the bladder is not wounded, as is the case in every other method of operating which has been recommended;

and, therefore, the great danger of exciting inflammation in that important organ, and through it, of producing peritoneal inflammation, is avoided, as much, as in the nature of the operation, it can be avoided. The parts which are divided, are less liable to increased action than the bladder itself; and if excited, the inflammation is more controulable and more easily subdued in those parts, than in irritable membranes.

These alone are most important advantages; but when to these is to be superadded, the great probability there is of permanently curing the patient, if the prostrate be not diseased beyond all hope or chance of recovery; it does appear to me, that the other operations should be laid aside, as most dangerous in their consequences, and as most inefficient in their most favourable terminations; being in all cases, only a *temporary* relief.

This last consideration will doubtless be attended to; and will have its proper influence on the minds of those gentlemen, into whose hands these papers happen to fall; and if they are induced, by what I have written, to try it, I confidently expect that they will think as favourably of it as I do.

In slight strictures of the urethra, where inflammation from any cause has arisen; or where, from ignorance or neglect, complete obstruction is brought on; and where there is not time allowed to have recourse to the caustic to open the passage; or where the patient is so irritable,

that even if there is time, the caustic cannot be applied; or where a stone is so wedged and impacted in the urethra as to block up the passage completely. In all such cases as these, the other methods of operating certainly may give relief; and the causes of the obstruction being of a temporary nature, and therefore being soon removed, either of them may possibly be serviceable; till the cause being removed, the effect ceases. But when the membranous part of the urethra is become so impervious that a pin would not pass through it, and where, as I have known, it has been impossible to trace it; or where the prostrate has been increasing for years, and the passage through it has been gradually obstructed and almost filled up, so that nothing will pass; what hopes can be entertained of more than *temporary* relief by puncturing through the rectum, or at the back of the prostrate, or above the pubis?

It is not pretended by any of their advocates, that by making either of these punctures the original cause of the obstruction can be removed. Why then should we have recourse to expedients only? Why run the risk of life for a mere *temporary relief*?

In the case of enlarged prostrate that Mr. Abernethy mentions, where he punctured above the pubis, we are not told any particulars. In this person, I should presume from the event, that temporary enlargement had come on from

inflammation having been excited; and that in consequence of depletion, which doubtless was had recourse to, the inflammation gradually subsided, and thus, in six weeks, the person got well. All I have to say is, that the man was fortunate; this fact does not in the least invalidate my objection to the practice.

The analogy which there is between this operation and that of lithotomy, although it was one of the most powerful reasons for my trying it, inasmuch as it proved the complete safety that there was in performing it, I am afraid will prejudice many surgeons against adopting it. To be told that an operation somewhat resembling lithotomy is to be had recourse to, would be enough to deter many men from attempting it.

Writers have delighted, one would almost think, in drawing up so terrifying a description of that operation; of adding to the difficulty which naturally attends it, a mystical something, which seems to make it improper and almost treasonable, that any, but the great and profound adepts in the art, should presume to attempt it. It certainly is therefore to be expected, that men who are awe-struck at the very name of lithotomy, will pause, and very seriously consider with themselves before they will attempt to do, without a grooved staff, what they have been told is so difficult to be performed, with all the advantages which that instrument affords them.

I do assure my readers, that they will not find such formidable difficulties in the operation as they may expect.

I must here request that I may not be misunderstood. I am by no means attempting to say that there is no danger or difficulty in performing this operation, or that of lithotomy.—I know them well.—I most deeply appreciate them.—But I mean to say, that there is not that excessive, that mysterious, and almost insuperable difficulty which has been conjured up, by some authors, I am sure with the very best intentions, as intending to guard young men against rashness and error; but by others it would almost appear, were little paltry interests possible to influence men on such occasions, for less worthy purposes.

Most certainly, no man should dare to perform either of these difficult and most important operations, without having dissected the parts, and without having made himself minutely acquainted with their structure and relative connexion. With this previous knowledge, joined to a steady hand and self possession, no one will find either of them insuperable. Indeed, without much anatomical knowledge, but with firm impregnable nerves, I knew a man who was surgeon to a large hospital, and who in all other respects was a wretched practitioner, to be one of the most adroit lithotomists that I have ever seen.

In performing the operation that I recommend,

it would be desirable that the patient should be secured as for lithotomy, though I have performed it with safety, without this tremendous preparation.

It is quite evident, that the person must be firmly secured by some means or other on his back. The catheter or sound being oiled, is to be passed gently down to the stricture or obstruction. If the obstruction is in that part of the urethra that is covered by the scrotum, all that is necessary will be to make a small incision through the integuments, and on the membranous part of the urethra being fairly laid bare, a small opening is to be made into that passage, which of course will empty the bladder. The urethra will be easily perceived, from the distended state in which it will be, from the straining of the patient, which necessarily takes place, from the stimulus of the operation, and from the bladder being so much overloaded with the urine. This will give the surgeon time to consider of what farther may be done to relieve the patient from the obstruction; but as what is proper to be done does not come under this subject, I shall only observe, that the surgeon must apply the proper remedies according to the peculiar circumstances of the case. In such cases, there is great scope for the resources of the surgeon to be displayed.

If the obstruction be not removed, it will be necessary to introduce a piece of bougie, or a

flexible catheter, into the opening which has been made in the urethra, to keep that part open till the passage is made pervious.

If the obstruction should be lower down in the membranous part of the urethra, the catheter or sound being pushed down as far as it will go, an incision should be made about an inch in length, through the integuments, immediately over the part where the catheter is felt. The urethra is to be carefully dissected to, so as to make it completely perceptible; which will be very easily effected, as the catheter or sound being in it, will prevent the possibility of error. The urethra being then opened longitudinally about a quarter of an inch, the catheter is to be withdrawn a very little, so as to admit of a probe being introduced, with the intention of being pushed through the obstruction. If the obstruction is caused by a mere stricture, this will frequently terminate the operation, as the bladder will be emptied, and the stricture may afterwards be gradually dilated by bougies, &c. till a catheter can pass; when the external wound may be permitted to unite; which it will very generally do without any unfavourable occurrence.

If, however, it is found to be impossible to pass the probe or the wire through the obstruction, there are then two methods of proceeding, viz. First, to dissect cautiously at the end of the catheter, gradually propelling that instrument forward as your knife advances, till at last, by the aid of these two instruments conjointly, the obstruction be passed.

Secondly, if it is found absolutely impossible thus gradually to creep on with the catheter, and to pass the obstruction, then, that instrument must be pushed firmly up to the obstructed part. This will give the precise direction of the urethra. The patient straining, as he unavoidably does in all such cases, will, if any portion of the membranous part of the urethra be pervious, so distend it, that by carefully dissecting in the direction of it, this tension will be discovered, the knife will be introduced, and the water discharged. Afterwards the incision may be enlarged, carefully cutting towards the catheter, which must be close up to the obstruction, and thus the passage may be recovered. This can sometimes be accomplished. It requires great care, and it is very tedious. But these are not to be considered, when we can, perhaps, restore an unhappy being to health and happiness.

When such cases occur, they must be considered fortunate, as it too often happens that the whole of the membranous part of the urethra is so contracted by long continued disease, that it is reduced to an almost impervious cord, with no possibility of tracing it; and very frequently the prostrate gland itself is so much enlarged and diseased, that not a drop of fluid can pass through it. These are terrible cases, and require the exertion of all our knowledge and all our firmness.

Even in these most deplorable cases, however, there is no occasion for absolute despair.

In either of these cases it becomes necessary to cut through the prostrate gland.

As I have described the mode of operating in those cases where, by great care and perseverance, it will be sometimes possible to recover the almost obliterated passage, I shall now proceed to state that which is necessary in the more complicated cases, where the urethra is so diseased and contracted, or the prostrate gland is so enlarged and thickened, as to block up the passage completely, and to prevent the discharge of the urine from the bladder.

In such cases as those which I have related, the opening through the integuments should be from half an inch to an inch in length, according to the extent of the disease.

When, however, the obstruction is in the prostrate gland itself, and it becomes necessary to dissect to it, the operator will be embarrassed if the external incision be less than an inch and half, or two inches; but in all other cases the extent of the first incision must depend on the part which is obstructed. It will likewise not be possible to state precisely where the incision should begin, when the obstruction is in the urethra, as it must depend on the locality of the obstruction. It will be enough to say, that of course no larger wound should be made than is absolutely necessary; and that we should divide the urethra as near the obstruction as possible, which will be ascertained by feeling the point of the catheter or sound.

When the obstruction is in the prostrate, the incision should commence as near the end of the catheter or sound as possible. The

handle of the sound or catheter must be held steadily, a little inclined to the right, to make it as distinctly felt as it can be in the perinæum, to the left of the raphe; and an incision being made a little to the left of that line, as nearly over the part where the instrument is felt as possible, must be carried obliquely downwards, nearly in the direction of the incision which is made in lithotomy. This oblique incision will carry the knife from the bulb of the penis, and from the rectum. The membranous part of the urethra being laid bare, the instrument in it will be a sure guide to dissect by to the prostrate gland. The fore finger being advanced, will enable the surgeon to dissect through the cellular membrane and the levator ani, whilst the fourth and little fingers depress and protect the rectum from injury. The rectum will be very certainly avoided by using this precaution, as there is not that risk of wounding it in this operation as there is in lithotomy, where the incision is carried so much further down between the anus and the tuber ischii.

On the prostrate being felt, and it is impossible in the adult living body to mistake any other part for it, as nothing near it gives the peculiar firm feel which it affords, a small opening should be made into the urethra close to it; by which an opportunity will be given of attempting to introduce a probe, or a female catheter, through the prostrate. This should always be attempted before the surgeon determines to divide the prostrate; it will sometimes succeed; but, unfortunately, it will be too often found impossible to push any

instrument through, without using such force as is likely to produce more injury than dividing the gland with a knife.

Exclusive of the immediate injury which violently pushing the catheter through such a firm body as the prostrate may be expected to produce; I should object to its being done when we are close to the prostrate, and can divide it with perfect ease; because, I think the opening which would be made by this forcible entry, would be very likely to close up again; and therefore, that this would be only a temporary relief. If, however, a catheter can be introduced without using much violence, certainly it should be done.

After having attempted to do this without success, it becomes necessary to divide the prostrate. To effect this, the knife should be turned a little obliquely upwards, and should be pushed from the urethra, not quite in a lateral direction, but with a slight degree of obliquity upwards through the side of the gland. This prevents the possibility of injuring the vasa deferentia, where they terminate in that eminence in the prostrate called *veru montanum*, or *caput gallinaginis*, which possibly might otherwise be hurt, were the knife to be passed laterally through the gland as in lithotomy, in which operation those parts are guarded by the staff being over them.

The rushing out of the water proves the knife having passed beyond the obstruction.

I can assure my readers, that they need not fear



any recession of the prostrate gland in consequence of the distension of the bladder carrying it up, as it is said to be, by one great writer ; nor need there be any fear of the gland receding, so that the knife will not pass easily and smoothly through it, as another great man asserts in his history of lithotomy. With deference to those gentlemen, I can positively assure my readers, that these are mere visionary fancies, and are not founded on practical observation. It is needless to tell a surgeon how he is to dissect to the prostrate gland. If he does not know how this is to be done, and where that gland is situated, and its relative situation to the rectum and bladder, it would be madness in him to attempt to perform this operation. If he does know the situation of this gland, and its connexions, and the course of the internal pudendal artery, I assure him, with care and steadiness, he will safely reach it.

There cannot in general be much hemorrhagy. Sometimes there is a branch of the pudic artery circling about the neck of the bladder. If it should unfortunately happen to be the case, a large tube wrapped round with lint, would, I should suppose, stop the bleeding if large enough to make considerable pressure. If it should not, I have not the smallest doubt that a tube large enough, and having some sponge tied round it, would have the desired effect. I must beg to say, that I state this from reasoning, and from what authors have written, as I have never seen it necessary. It would be desirable to use lint in

preference to sponge, if it can be effectually used; as it is very difficult and troublesome to remove the latter from any situation after it has been applied a few days, and it must be peculiarly so from this.

The patient being carried to bed, a slight dressing of lint may be applied over the wound, but this is of little consequence, as whatever is applied will soon become wet from the oozing out of the urine.

After the first operation of this kind which I had, I was very anxious to introduce a flexible catheter through the penis, and beyond the incision into the bladder, and to keep it there; as I thought a good deal depended on this being constantly attended to. But I now know that this is not absolutely necessary, and indeed if it were, it would be almost impossible to effect it, as it gives the patient great uneasiness, and it is continually slipping out of the wound in the prostrate.

After the fourth or fifth day I think it is serviceable to introduce a tube made long enough to pass beyond the external wound, and having the end open, not rounded and pierced with holes, as the catheter is. The wire or stilet should have a bulbous knob affixed to its end, to fill up the end of the canula, so that it may pass down the urethra smoothly. This tube should be passed down to draw the urine off; which it does more completely, and with less leaking than the catheter. I have used this always, and I have thought it facilitated the cure. But perhaps this is but a

trifling, unnecessary refinement, and most likely the catheter will do quite as well.

If upon trial it should answer the purpose it should be preferred, as the multiplication of instruments is an evil, and the more simple all our instruments are, the better it is. The object is to procure the desired effect by the simplest means.

Although it is a material object to procure the reunion of the divided parts as quickly as can be done with propriety, I have never attempted to do this by the first intention, as I think I have heard has been done after lithotomy. I should apprehend, were this possible, that if the edges of the external wound healed too rapidly, and before the new formed passage had adapted itself to it's office, that some mischief might ensue from effusion into the cellular membrane; and therefore, that sinuses might be formed.

No alarm need be felt about union taking place if all goes on well, and if the habit of the patient be tolerably good. The time when this will be accomplished must depend on many circumstances, as is well known after the operation of lithotomy.

The after treatment must be similar to what is required in that operation, and therefore need not be particularly detailed.

As I have said, I was at first anxious to keep the parts open by inserting catheters, &c.; but now I have learnt that the passage of the water will effectually do that; and therefore, it will not be necessary to trouble the patient with too frequent introduction of any instrument. It may,

I think, facilitate the cure, to introduce a canula such as I have described, or a catheter beyond the wound, after the fourth or fifth day, whenever the patient feels the desire to empty the bladder.

Before I conclude this paper, I must observe, that in several cases that have occurred to me, it has happened, that after the parts have united the person has discharged the urine in a full stream, when, for years before, he had never discharged it but with the greatest difficulty, in a very contracted stream, and in some instances merely by drops.

In fact, from the resemblance of the incision to that which is made in lithotomy, it was to be inferred a priori that it would be so; and I am surprised, that from analogy, it had not occurred to surgeons to make trial of this plan of operating long since.

I cannot help observing, before I conclude this paper, that I have never performed this operation without reflecting on the different methods of operating for extracting the stone from the bladder; and when I have reflected on the facility with which I can dissect safely to the bladder, without the assistance of a staff to guide my knife, I never have thought on that subject without a feeling of astonishment, that surgeons should be so wedded to the use of the gorget as some of them still are. I cannot help believing that this arises in some from a defective knowledge of the anatomy of the parts; in others, from that terror with which their minds are filled

by the mystery which some writers have attempted to shroud this operation in ; and in others, from prejudice in favour of old habits. This last, I know, it is difficult to conquer. There is however, in my opinion, much greater danger to be feared from the use of the gorget. I have seen sad work made even in some of the great hospitals in London from the slipping of that instrument between the bladder and rectum, and I have heard of many others.

I have briefly related the first case in which I opened the urethra. I believe with more firmness, I might safely have forced the catheter through the obstruction. But at that time I was absurdly timid, and I believe that I operated when there was no necessity for an operation.

The second case that occurred, was that of a person, who, from having laboured under strictures in the membranous parts of the urethra for several years, and having discharged his urine in very small quantities, and at very short intervals, at last became so bad that he was sometimes more than an hour in emptying his bladder by drops only at a time ; till at length a complete suppression took place. He had been almost three days without having passed a teacup full of urine. The bladder was prodigiously distended, and was felt hard and tense above the pubis. The man was in tortures. I bled him largely, and tried the warm bath ; dashing of cold water on his belly and legs and thighs ; the muriated tincture of iron was likewise given, but in vain. Silver catheters ; elastic catheters, and catgut bougies of

all sizes were attempted to be introduced, but all without effect. It became necessary to decide immediately what was to be done. The wretched man was ready to submit to any thing that was suggested, which promised to release him from his agonizing sufferings. I determined to cut to the parts. He was laid on his back and firmly secured. The catheter was passed down as far as it would go. I divided the integuments in the manner, and to the extent, which I have before mentioned; and on laying bare the urethra, I withdrew the instrument a very small distance before I divided it, and then made a small opening close to the stricture. I then attempted to pass a small probe, and afterwards a wire through the obstructed part, but without success.

I dissected then with great care towards the prostrate, and tried to discover the distended urethra beyond the obstruction. But it was so contracted that I could not feel any trace of it. I completely cut to the prostrate gland, and endeavoured to find the urethra as it issued from that part, which I expected certainly to discover. I was again foiled. As I now distinctly felt the prostrate, I determined to push the knife through it. I did so, by slightly turning the knife upwards in an oblique direction, and pushing it through, beginning as near the part that the urethra issued from as I could ascertain. The man was relieved; but I must confess that I did not then expect that he would be permanently cured, for the whole of the membranous

part of the urethra was impervious and useless; it could not be discovered that such a canal had existed; and I knew nothing from reading or from experience that warranted me in expecting any thing more than a temporary relief, and at last, an incurable fistulous opening in the perinæum.

I had however saved the man from a wretched death; and I should willingly have compounded for this inconvenience, as the parts in time would certainly have become callous, and it would have served the purpose of discharging the water from the bladder.

I assiduously attempted to keep in a flexible catheter through the penis; and bending it up, I tried to keep it in the wound of the prostrate. This I found impossible. I then tried to keep it in the prostrate by passing it through the opening which I had made in the perinæum. I teased the poor fellow very much to attend to this in my absence; but he neglected it, or he could not succeed in doing it. I then contented myself with passing a flexible catheter through the penis, and a little way beyond the incision in the perinæum. This I contrived to do, and to keep it there for some time, without great inconvenience to the patient.

On the fifth day, by pressing on the external wound when the person felt an inclination to discharge his urine, a good portion of the water passed through the catheter.

As the man complained very much of the ca-

theter being continually in the urethra, I made him try to introduce it occasionally himself. On the tenth day, some water passed through the urethra, without the catheter having been introduced. I then thought I was interfering unnecessarily, perhaps indeed injuriously, poking with the catheter, and began to think with much surprise, that a new passage was forming. I however did sometimes very cautiously introduce it, for I could not quite confide in this operation of nature; so obstinately do we sometimes blind ourselves by our preconceived opinions and prejudices.

I desired my patient always to press on the perinæum with a soft compress when he wished to discharge the urine; and thus going on, in about a month the external wound united, and the man emptied his bladder by the natural passage freely, without let or obstruction.

I have perhaps too minutely and tiresomely detailed these particulars; but, perhaps, I may be excused, when it is recollected that it may save others from too officiously interfering with the quiet operation which nature was effecting, by mischievously teasing the man by the introduction of the catheter. I know I did so. I could not, however, succeed in keeping it in as I wished; and nature effected without my assistance, or perhaps to speak properly, in spite of my improper interference, all that I hoped to do.

I have not mentioned the daily medical treatment, in reading which, as dragged out by some

writers, I have many times been sadly tired and disgusted with long prosing transcripts from their daily journals. It must be evident, that in such cases, a low diet and strict abstinence from fermented liquors must be enjoined; the bowels must be kept carefully open; and feverishness, and heat, and restlessness controuled by all the means in our power.

This case opened to me an unexpected event; for I could not, and I do not, doubt, that a new passage was formed from the prostrate to the healthy portion of the urethra. I should not have felt any surprise at this having happened where the prostrate only was opened. So far I expected would take place, from the analogy of the operation for the stone; but the circumstance of a new and useful canal being formed, instead of the ruined and obliterated urethra, I could not have hoped.

I mentioned this case to many surgeons. It was received with great scepticism. They thought that I had deceived myself, and that in drawing my knife from the prostrate longitudinally along the course of the urethra, that I had fortunately hit upon the natural passage, and thus that I had divided the stricture, and that the parts afterwards recovered, as they do after the operation for the extraction of the stone.

I was almost persuaded, by their reasoning, that this was the case; and I began to think that by poking with the catheter to keep the parts open as I intended, that I had mischievously in-

terfered, and had prevented the parts from uniting so soon as otherwise they would have done if they had been left to themselves.

At this time a case occurred that threw much light on the subject. A young man called upon me, informing me that he could not discharge his urine. I introduced a catheter with some difficulty into the bladder. The obstruction was about the neck of it. I desired him to go home, and keep himself quiet, to live low, and I gave him some opening medicine to take. He sent for me early in the morning to draw off the urine, as he could not empty the bladder; and indeed he had not passed half an ounce since I had seen him the night before. I again introduced the catheter, but with increased difficulty. On withdrawing the silver catheter, I immediately introduced a flexible one, and left it in the bladder, as I was afraid that there would be a good deal of difficulty in this case. In the night, whilst he was asleep, it escaped from the bladder and he could not re-introduce it. He sent for me, but as I was from home, and did not return till noon, he became very impatient and sent for my assistant, who endeavoured to replace it in vain. Being a person of great resolution, very impatient, and suffering intense pain from the distension of the bladder, he withdrew the elastic catheter, and tried to introduce a silver one, which I had left in his room. Being foiled in attempting to pass it by gentle means, he tried what force would do; and used such violence as to break the silver

catheter in the convex part of it, not into two pieces, but so as to crack the curved part of it.

I saw him soon after. There was a little discharge of blood, but no hemorrhagy of any consequence. He did not complain of much pain in the injured part; the great pain from the distension of the bladder superseded and absorbed all other feelings and considerations. I introduced another catheter, and attempted to regain the proper passage; for it was evident that he had passed the catheter through the side of the urethra, and I supposed it must have been stopped by the prostate gland; as nothing else was firm enough, in that situation, to have given such resistance as to have broken a catheter;

I made several attempts to find the proper canal in vain. The bladder had been now so long distended, that it became urgently necessary to relieve him. I determined therefore to let the catheter pass along the passage which he had formed, and carefully introduced it, till it was stopped by a firm body. As no urine passed, I introduced my finger into the rectum to ascertain where the point was lodged, and plainly traced the catheter entering a considerable way into the prostate.

His brother, who is a most respectable manufacturer, was in the room, and seeing me hesitate, and apparently at a loss what to do, proposed to me to call in a very excellent practical surgeon, now dead, as I was little known to him, or in-

deed to any person in the town, it being very soon after I came to reside in Birmingham. I am always most happy in consulting with men of experience and knowledge in difficult cases; but I declined it in this particular case, as I was apprehensive, that I should have been thwarted in doing what I thought necessary to be done, and which I did think would have been to have cut through the prostrate.

As however I found so much of the prostrate already penetrated by the catheter, I concluded that all the mischief which could be done, was already effected; and therefore I determined to force the instrument firmly and steadily on, through the gland into the bladder. My finger being in the rectum, gave me a point to act against, in pressing the catheter upwards into the bladder. It happily passed into that cavity, and the urine rushed out through the catheter. I almost trembled at my success; and I was at a loss what was next to be done. It seemed evident, that the catheter must be kept in the position in which it had entered; and this was carefully attended to; indeed so carefully, that from having used a catheter which was brought from a Sheffield instrument-maker, on withdrawing it several days afterwards, I found that above two inches of the caoutchouc had been detached, separated, and left behind in the passage. This was a new disaster. It fortunately was of short duration; for after blockading the canal for some time, and alarming me with the terrors of a new operation, to be performed to remove the cover-

ing of the catheter, which I accused myself as having caused, by my over anxiety to keep the new passage open; it was dashed out with a flood of water; and removed all my fears for the effect of the caoutchouc, and all my doubts about the permanence and openness of the new formed canal.

The pain and inflammation from the injury inflicted on the prostate, were not so great as might have been expected; depletion was had recourse to; a strict avoidance of all cause of irritation was enjoined; and this gentleman is now, thirteen years after this event, perfectly well; his water having been discharged ever since, freely and without impediment.

This case is strongly illustrative of what Mr. Dease mentioned to Mr. John Bell, as being his practice in obstructions of these parts; and as far as one solitary case can, supports the safety and efficacy of his practice.

This was a recent disease. The prostate gland was sound and of its natural size. The force that was necessary, even in this case, was such as to crack the catheter. I should not dare to make the attempt in an enlarged state of the prostate; and I should doubt, even after the instrument had passed into the bladder, whether the passage would continue pervious.

Exclusive of this, there would be great difficulty, as I found in this case, to keep the instrument in the new made orifice; and if it once slipped out, before the parts were become agglutinated and hardened, it would not be a very easy

affair to regain the opening which had been made through the urethra, and afterwards through the prostrate. This gentleman complained of great pain from the length of time which it was necessary to keep the catheter in the urethra; and if any alarming symptoms had come on, I must have withdrawn it. For several days I thought that I must have done so, as he complained of great pain about the neck of the bladder; but being a person firm, and of unusual courage, he bore it, on my explaining to him how much depended on the instrument being retained.

From what I saw in this very favourable case, I should be fearful of recurring to the practice, except under similar circumstances.

That a new passage was formed here, is proved beyond all doubt. I suppose by inflammation coming on, coagulable lymph was effused, which glued up the cellular membrane, and thus consolidated the parts, preventing the effusion of the urine into the surrounding membranes.

This case confirmed me in the opinion which I had entertained, till I was driven from it by the observations of others, viz. that in a former case which I have mentioned, I had succeeded in forming a new passage, to supply the place of the spoiled and ruined part of the urethra.

However this may be, I have not fondled this bantling of my imagination so long, or so ardently, as to feel quite a paternal affection for it; and if it should be proved to be an illegitimate and worthless one, I can, without many pangs, con-

sign it to oblivion ; content, if I shall have pointed out a method of relieving a dreadful complaint, however I may have erred in accounting for the effect produced.

Since that time, three other cases have occurred, in each of which the obstruction was in the prostrate. In one of these the prostrate was very much enlarged, and pain had been felt in it for several years before ; and the urine had been discharged with great difficulty. In the other cases the prostrate was considerably increased in size, and the difficulty of evacuating the bladder had been very great ; in one case for more than four years, and in the other for seven.

In two of these I performed the operation with success. In the other, the bladder had been distended nearly four days, and only a few drops of water had been discharged. After the water was let out, the tension of the belly continued and was never reduced. The pulse continued at 130 in a minute. Great pain and tenderness in the abdomen never ceased. The tongue was parched. Shiverings were alternated with flushing heat. Restlessness and delirium came on, and death closed this distressing scene. It cannot be doubted, I believe, that this person died from the distension of the bladder having been suffered to remain too long, which had brought on inflammation of that viscus, and which had extended to the peritoneum.

In every one of these cases I carefully tried to force a female catheter through the prostrate ;

but the gland was in each of them so much increased in size, that I could not have effected it, without using such violence that I did not think I was warranted in having recourse to.

I do not think it necessary to give the detail of these cases, nor of others where I have cut into the membranous part of the urethra for obstructions in that part. The operation in each has been on the principle which I have related, and it has been performed in the manner which I have described.

Cases of this kind are situated in such parts as to preclude the mention of the names of the patients. I have, however, the satisfaction of recording one case, where, although the name cannot be given, the operation was performed from my explanations, and in my presence, by another surgeon, and where precisely the same beneficial result was obtained.

This is the case of a respectable tradesman of this town, about sixty years of age, who had laboured under partial obstructions for thirteen years. The obstruction was so great as to make his life very uncomfortable. At times, his urine was discharged only by drops; and at all times with the greatest difficulty. From some cause or other, he at length became incapable of discharging it in any way. Mr. Partridge, surgeon, of this town, was called to him. That gentleman used all those means which are recommended in such cases, without effect; and I was requested to meet Mr. Partridge.

I found that the person had been three days without passing any urine which could relieve him. He had passed, with immense straining and difficulty, a few drops, as almost always happens, and which too frequently leads to very fallacious and dangerous hopes in the patient, and too often, the surgeon is misled by the same delusions. I have seen cases where this trifling discharge has induced the patient and surgeon to expect that all difficulty was nearly conquered; and has induced the latter to defer proposing the operation, from this trifling proof of the passage not being completely obstructed. The wretched patient and his friends are always willing to catch at any thing which can give them any chance of postponing the time of operating; and I have seen instances, where, from this mistaken humanity, such inflammation and mortification have come on, as to render the operation quite useless.

But to return. I found this poor man in the extremest pain, restless, changing his posture momentarily, in the vain hope of relieving the torture he was in, and expressing the greatest anxiety to be relieved by any means that could be thought of. As Mr. Partridge was obliged to see another person before we met, I took the liberty of trying to introduce the catheter; but I could not succeed with any one of any size, nor with an elastic catheter, nor with a catgut bougie.

The person was bled, and placed in warm

water, and while in the water a catheter was attempted to be passed. Cold water was then dashed upon his belly and legs.

As every method had been tried in vain, which was likely to relieve him, the operation was proposed to him; and such was the dreadful pain which he had suffered, that he determined to submit to it without a moment's hesitation.

Mr. Partridge withdrew with me into our patient's brother's house while the necessary preparations were making. That gentleman enquired, whether I recommended the operation of puncturing through the rectum. I explained to him my objections generally against that operation, and particularly in this case, as I had ascertained that the prostate was enlarged; and from the length of time which the disease had been coming on, I was quite sure that we should have a formidable disease to contend with.

I pointed out the utter impossibility there was of the puncture per anum affording any thing more than temporary relief. I stated, that the formidable disease, the blocking up of the canal through the prostate, would remain unaltered and unalterable by the puncture through the rectum. That we should not have advanced, after the operation, one step towards our object of saving our patient, and we could not, conscientiously, hold out a hope to him or his friends of a cure. I then begged leave to state to him what I had done, and with what success the operation

which I had recommended had been performed; and further urged, that the danger of this mode of operating was not greater, indeed not so great, as either of the others; as in those of puncturing through the perinæum, or anus, or in cutting above the pubis. That we should avoid all their inconveniences; and if we succeeded, we should in all probability *completely cure* our unhappy patient.

Mr. Partridge most obligingly listened to my long harangue; and with great candour, and without hesitation, determined to adopt the plan which I recommended.

As I had never had the satisfaction of seeing Mr. Partridge operate, I took the liberty of speaking thus at length to him, as he was going to perform an operation that he had only heard of then for the first time; and, therefore, it was impossible that he could know any thing of it but what I had mentioned to him; and, consequently, that although he might be, as he proved himself to be, a good anatomist, that he might not have minutely studied those parts.

I requested him to try to get beyond the obstruction if possible, as I thought at first that the obstruction was in the middle of the membranous part of the urethra.

The person was secured on his back, as for lithotomy, and the integuments having been obliquely divided, and the urethra laid bare, Mr. Partridge tried to cut beyond the obstruction;

but he found the canal beyond it so completely obliterated, that he could find no trace of it; and therefore he proceeded to dissect onwards towards the bladder. Having reached the prostate, he requested me to examine it, and on ascertaining it, I requested him to pass the knife, with its edge turned obliquely upwards, through the upper side of it, as otherwise the terminations of the vasa deferentia might have been divided; as it must be recollected, that the urethra does not pass through the centre of the prostate, but through the upper part of it. This being done, the urine of course flowed out. The person was placed in bed; having the wound covered slightly with some lint. The next morning, as the urethra was so completely obliterated, I was anxious to introduce a flexible catheter into the bladder through the penis. I did this; but it was found impossible to keep it there. We were obliged therefore to content ourselves with keeping it as far in as we could. The end of it was therefore continued to be kept as far beyond the incision as was possible; but it caused such irritation, that it was frequently obliged to be removed, and in a few days urine passed through it. I must again beg to remind my reader, that this keeping of the flexible catheter in the part may be dispensed with. It produces great pain and irritation, and the more cases I see, the more I am convinced that it is more than useless. It certainly should be occasionally introduced.

For the first three days our patient was in great danger. His pulse was excessively quick, his tongue and skin were very dry; he was very hot, with thirst and head-ache; his belly was tense and tender. Castor oil was given him, as he was costive; and antimonials to relax the skin; and he was kept on the very lowest diet. I do believe that these symptoms chiefly arose from the long continued distension of the bladder, which had brought on inflammation, or symptoms of irritation, as all these symptoms appeared before the operation, and certainly so important an operation as this must have tended to have increased them. After the fourth day the pulse became slower, the skin became moist, the tenderness of the belly decreased, and in six days the danger was at an end. On the eighth day there appeared a white covering over the incision, not at all like a slough, but as if it were a deposition from the urine. As we thought it might be a slough, we gave him the bark, and Mr. Partridge some days after ascertained that it was a sloughy membranous substance, as he separated it in dressing the wound, the lips of which it left clean and healthy.

The patient had a tube made open at the end, and with a bulbous knob attached to the wire or stillet, which he introduced beyond the incision when he wanted to discharge his urine; this in a short time became useless, as the external wound gradually closed, and the water was im-

pelled in an ample stream through the natural passage. I have seen this person a few days since, and he is perfectly well; the water flows unimpeded; and he has never had the smallest obstruction since the operation was performed, which is now fifteen months.

I think this case, with the others that I have mentioned, prove beyond all doubt, that a new canal was formed. The urethra, in this case, was so obstructed and impervious, as to become undistinguishable from the surrounding parts; and there is no other mode of accounting for the complete cure which followed the operation.

It gives me great pleasure that another surgeon operated in this case, and that I had never had any conversation with him on the subject before; as it proves that any surgeon, with a competent knowledge of the parts, may easily perform this operation. It does away all the objection which some have urged against it on account of its danger and difficulty.

I ought to add, that Mr. Partridge performed the operation with the greatest steadiness. In the different stages of it, he requested my opinion as to the manner of proceeding; as it was not possible that a person who was suddenly called upon to perform such an operation, in a part of the body where the life of a human being depended upon nice dissection, and which was to be made merely by the aid of his finger to guide

his knife, could possibly go on with such confidence as he would have done if he had performed it before.

He likewise laboured under the great disadvantage of performing this operation immediately after he had heard of it for the first time. I tried to convey to him, as clearly as I could, what I wished to be done, and he ably carried it into effect.

From what I have related, is it not proved, that to puncture through the perinæum, or through the rectum, or above the pubis, is bad practice in all cases of obstruction to the discharge of the urine, which arise from any other than temporary causes?

If I have not deluded myself, I think I have established, that the method of operating which I have described is safe and effective. Safe, comparatively speaking; and effective, if the patient live. That it always will succeed I do not flatter myself; but it appears to me to promise more complete success than any other method with which I am acquainted.

Of the danger of the operation itself, all surgeons are competent to decide. The operation is somewhat similar to lithotomy. The same parts are divided; and although it is more difficult to perform, especially in those cases where no instrument can be introduced into the urethra as far as the prostate, it certainly may be performed with safety. There certainly is in

some cases great difficulty ; but I re-affirm, there is none but what can be overcome ; and where the object which possibly may be attained is considered, viz. a complete cure of one of the most afflicting diseases which man is subject to, I think it my duty urgently to recommend it to the consideration of the learned and scientific members of our profession.

On removing tumours from some of the accessible cavities of the body.

ON REMOVING ENLARGED TONSILS.

IT is well known to all surgeons, that the tonsils are subject to permanent enlargement; that they frequently are so much increased in size as to render deglutition difficult; and sometimes to impede respiration to so great a degree as to threaten suffocation. When they are so much enlarged, it becomes necessary to remove them, or so to reduce their size, that those important offices of life shall not be farther interrupted. It is likewise important that they should be removed when they are much enlarged and are disposed to become hardened or schirrous; because, in that state, they are subject to inflammation from the slightest application of cold; and from frequent repetitions of inflammation, at length such a disposition to ulceration is induced, as sometimes to destroy life.

Ulceration, however, does not often take place. The parts, although so constantly liable to inflammation, resist the ulcerative process for many years, and even for a long life, with little increase of size, and with no other inconvenience than a

difficulty of swallowing whenever they are inflamed.

I believe many medical men are of opinion, that these enlargements, never take place but in scrofulous habits. This is certainly a mistake, as I have frequently seen them where no trace of scrofula could be discovered either in the person or in the family. This disease is certainly most frequently met with in persons of fair complexions, and women are more subject to it than men. I have never seen but one case* in males, where the tonsils were so much enlarged as to make it necessary to remove them. This was in a child of three years of age, whose case I shall relate. This child was, I believe, born with the tonsils preternaturally enlarged.

When these tumours become so large as to interfere with the acts of swallowing or breathing, or where there is a disposition to ulceration, there can be no doubt about the propriety of removing them; and I shall be able to demonstrate, that this can be done with the greatest ease, and with perfect safety.

There have been many different methods recommended for extirpating or destroying enlarged tonsils. Some writers have recommended the application of different caustics to be applied to them to destroy them; and I was acquainted with

* Since writing the above, a gentleman from the neighbourhood of Shiffnal, in Shropshire, has consulted me for great enlargement of the tonsils, and which must be removed if they continue to increase in size.

a surgeon of considerable reputation, who recommended the frequent application of a hot iron to them, conveyed to the parts through a canula, to protect the tongue and lips. He spoke confidently of his success; but I should scarcely expect that any person would follow his practice.

The application of caustics is liable to much objection; and I cannot conceive how it can be effectual. They can only be partially applied, and at long intervals. If they are applied daily, it is evident they must be applied unnecessarily; as their action would be prevented by the slough which they had previously produced; and when that slough had separated, they could not be again applied till the inflammation which had been excited had subsided. Exclusive of this objection, it must be recollected, that, in those cases which require extirpation, the caustic can only be applied to a small portion of the substance which is to be destroyed; and from the great size of the gland in those cases, and that the back part of it is quite out of the reach of the caustic, the surgeon, by poking with his caustic in the throat of his patient, may torment, without relieving him. He may, indeed, excite ulcerative action, and thus seriously affect his patient's health; but it is not possible for him, by such means, to remove the diseased gland.

I particularly state this, because I know this absurd practice has been frequently resorted to; and in one case which came under my care afterwards, brought on very alarming symptoms. In

that case it had been persevered in for several weeks, till the young woman insisted on leaving an hospital where she had been for the cure of this disease.

As surgeons have been foiled in removing them by caustic, excision has been resorted to. I have once cut out a diseased and enlarged tonsil, and I can assure my readers, that it was a very alarming operation. The bleeding was not from a vessel that could be got at by a tenaculum, or needle, and tied, or to which sponge could be applied. I had recourse to styptics of various kinds, which were washed off without the least effect. Caustics were applied, but did not suppress the hemorrhagy, and nothing but the application of a hot iron had any effect. I have never attempted this operation again. And yet writers recommend it. A very able writer, and from what I have observed of his writings, I should suppose as able a surgeon, Mr. Want, has lately recommended this mode of removing them. In the *Medical and Physical Journal* for December, 1814, Mr. Want tells us, that it is never necessary to strangle tonsils, as they may always be safely cut away with a pair of curved scissors; and that the hemorrhage afterwards is always trifling. I know not how to account for this, when I recollect the hemorrhage that I witnessed after my operation. Perhaps Mr. Want's method of removing them, by squeezing them off with scissors, may account for this. I have been told by surgeons who have cut them off, that they have met

with as alarming a bleeding as I experienced. Be this as it may, I am confident that I have met with cases where the tumours were so large and hard, that it would be scarcely possible to force scissors through them in the short time that it would be possible to keep the scissors in the throat. The irritability of those parts is so great, that no person, I should think, could possibly preserve their firmness and self command, while the blades of the scissors could be introduced so far above and below the gland (when so large as to fill the whole cavity) as to enable the surgeon to include the whole, down to the very base. The division too must be instantaneous (which, in such tonsils as I allude to, must be most difficult) or the gland cannot be perfectly cut through. But supposing all this possible, which I conclude it is, from what Mr. Want says, this can only be applicable to adults. In children it must be impossible; and in infants I know the necessity of removing them sometimes occurs.

Exclusive of these, which in some cases appear to be insuperable objections, few persons, I should suppose, would consent to the use of cutting instruments, when they were informed that the tumours could be removed safely and certainly without them.

As to the dangerous erysipelatous inflammation which Mr. Want mentions, as frequently arising from the application of ligatures; in my practice I have never seen it. I know nothing of it. I assure Mr. Want that I have removed many,

and I have never seen any dangerous inflammation of any kind, nor any other kind of danger. The process is certainly tedious, but it is safe and effectual.

What may arise from piercing the gland I know not, as I have never operated in that way.

As surgeons became alarmed at the hemorrhagy which had been said to attend excision, various engravings have been given in almost all systems of surgery, of different instruments which have been invented to remove these tumours by strangulation.

But they are all, at least all that I have seen, too complex to effect the purpose they were intended for, but with the greatest difficulty. The ring of Hildanus, which is the basis of all the others, is quite a serious piece of machinery. The ligature certainly can be fixed and tied on the tonsil by the aid of that instrument; but I am sure, from considerable experience in operating on that part, that it must be done with the greatest difficulty; for what is done in so irritable a part, must be done quickly. But even if the ligature can be tied on the enlarged tonsil, it can only remove as much of the gland as it strangulates; that is, only the superficial or external part of it. The interior of the gland cannot be pressed on; and consequently it would live, being nourished by its central vessels. The operation must therefore be had recourse to, again and again, to destroy the whole.

The great Cheselden recommended to pierce

through the gland at its basis, with a pointed instrument, having an eye near its end, in which was conveyed a double ligature. This ligature was then to be cut; and by another contrivance, the gland was to be tied in two parts by the divided ligature. This plan would succeed; but it seems to me to be almost impossible to perform it. Mr. Sharpe likewise recommends this method of proceeding.

There have been many other contrivances in ancient and modern times.

One modern writer of a system of surgery recommends a silver wire to be introduced through a double tube; and thus prepared, it is to be introduced through the nostril, and to be passed round the tonsil, which is thus to be strangulated; and the wire being tightened from day to day, the tumour will die, and fall off.

I feel quite certain that this never has been done. The author, however, very gravely and minutely proceeds to describe how this is to be carried into effect, as if it was an operation that he had frequently performed. Whoever considers for a moment the relative situation of the tonsil and the nose, will at once comprehend the utter impossibility that there seems to be of noosing a tonsil in the manner which this writer recommends; and even if it could be accomplished, the wire would inevitably cut through and destroy the velum palati.

A writer in the *Medical and Physical Journal* for December, 1814, Mr. Walker of Oxford, re-

lates a method which he has practised. It seems to me to be too difficult to be applied effectually.

Mr. Chevalier, of London, has published a paper in the 3d vol. of the Medico Chirurgical Transactions, on this subject. He says, "that passing a ligature round a diseased tonsil has been found an operation of considerable difficulty. The breadth of the tumour at its base, and the extreme impatience of the fauces, concur very much to embarrass the operator."

This is certainly very true, according to the common mode of operating; and with great deference to Mr. Chevalier, I think he has not succeeded in rendering it more simple. His plan differs very little from that which has been long since recommended by Mr. Cheselden and Mr. Sharpe, and others, viz. perforating the gland in the first place, and then strangulating it, by including it in a piece of thread tied by a peculiar knot. His mode of tying the knot certainly appears easier than any other that I have read any account of. It is, however, much too complex, as all the others are.

I can assure Mr. Chevalier, if he will once try the method which I recommend, he will find, that of whatever shape the gland may be is not of any consequence, nor whether it be attached by a broad base, or by a mere pedicle, as the tonsil can be easily removed in either case; nor will he find the tube remaining in the mouth to be any thing more than a trifling inconvenience.

From what I know of removing tonsils, I am

utterly at a loss to conceive how it is possible to pierce the gland, and then to introduce a ligature through it, and afterwards to tie this ligature, when I recollect what convulsive action is produced in this irritable part by the simple plan of operating which I have adopted.

Although various methods have been pointed out by surgeons for removing enlarged or diseased tonsils, it is surprising how many medical men there are, many in very extensive practice, who are absolutely ignorant of any method of removing these glands. I have been told by surgeons of great respectability, that they have never attempted the operation; that the methods which have been described by different authors, seemed written merely from theoretic reasoning; and that they did not think any of them practicable. I believe this to be a very general opinion. If I can do any thing to remove this error, if any thing can be pointed out to simplify this operation, I trust it will be of some little service to our profession.

It may be, however, necessary to observe, that I have heard that some physicians have taken upon them to decry this operation. It has been asserted, as I am informed, that a very learned man has declared, that the disease is constitutional; that it is not local; and, therefore, the mere removal of the enlarged glands is doing nothing. That it being a general disease, general remedies are necessary.

This I most positively deny. I have removed tonsils in every stage of disease. I have never

used a single grain of medicine in any case, excepting during the time of the operation, when I have given an antimonial to keep the skin moist and free from fever ; or an opiate to produce rest. I have never used any alterative medicine, nor any other to influence the general habit, and in no single instance have I ever seen a return of the disease. I know that it is supposed to be connected with scrofula. I have never been able to trace it to that disease. Certainly, I have seen enlarged tonsils in scrofulous children, which is not surprising, when it is recollected how much the whole glandular system is affected by scrofula. I can very truly affirm, that if the tonsil be once fairly removed, close to the base, that there will never be any return of the tumour. Sharpe told us this long since. He supposed the disease to be of a cancerous nature, and says, " this disease does not return like other schirrous tumours."

From having been fortunate enough to remove the tonsils in two very bad cases, several years since, it has fallen to my lot to have had many applications from persons who have had them enlarged. Some I have removed from mere enlargement, to obviate the mechanical inconvenience they created from their size only. Others I have taken out in consequence of ulceration having begun in them. I have never failed to cure the patient even in the ulcerated stage, if the ligature could be applied below the disorganised part; and I re-assert that the disease is merely

local, and by removing the glands the patient will be perfectly cured.

Having thus taken a view of what has been done by others for the removal of these tumours, I shall now state the method of operating which I have adopted, and which I have used in a great number of cases in this town and neighbourhood, with invariable success. The simplicity of it will, I think, sufficiently recommend it; and with common adroitness, it will be found completely effectual.

The same instrument I have used for the removal of tumours from other cavities; and by occasionally changing the kind of ligature according to the parts from which substances are to be extirpated, and according to the size of the tumour, it will be found the most effective, and the most simple in its application, of any which has been heretofore described.

The instrument is merely a double silver tube, about three or four inches long, according to the age of the patient from whose throat the tonsil is to be removed; through this a well annealed silver wire is to be drawn, so as to leave a noose at one end, of such a size as to enable the surgeon to pass it easily over the enlarged tonsil. The tongue being depressed with a spoon by an assistant, and in some patients, who have not such firmness as to keep themselves still, and their mouths sufficiently open, the mouth being propped up or gagged, by inserting a piece of soft wood between the teeth, of a size sufficient

to keep the jaws distended; the surgeon holding the tube in one hand, must apply the noose of the wire over the tonsil, carefully observing to slip it up close to the very base of the tumour; he must with his other hand instantly draw the wire tight, and so tight as to draw it slightly into the substance of the tumour, and bending the wire over the side of the tube he will effectually prevent it from slipping off; and thus the tonsil will be completely strangulated. The tube is to be applied against the angle of the mouth, where it will be retained with much less inconvenience than might be expected. By drawing the wire sufficiently tight, so as to make a little impression in the substance of the gland, the possibility of its being thrown off, by the convulsive straining which always takes place on the instrument being fixed, is obviated.

There is some dexterity required in placing the wire quite at the base of the gland, close against the side of the throat, and immediately drawing it tight enough to fix it there. But with care this is not difficult. It certainly must be done quickly, as the parts are immediately excited into spasmodic action; and if it be not instantaneously fixed, the instrument must be withdrawn till the person is become composed; as the violent retching and straining which are brought on, are alarming to the bystanders, and terrible to the patient, and render any attempt to fix the tube perfectly nugatory during the continuance of the convulsive action of the parts. I have never failed to

fix the instrument on the second or third attempt, even in the first cases that I had the care of; and I can generally, now, strangulate the gland on the first trial. Indeed, I feel quite certain, that I could strangulate a tumour of this kind, even if it were conical; as by instantly drawing the wire tight, when it is perceived against the side of the throat; or if the patient should be uncontrollable and shut the mouth, when the wire is felt against the fauces, which is perceptible enough, the base being easily compressible, is so indented by the pressure of the wire as to be permanently fixed.

There is some little nicety required in the choice of the wire; if it be too thick it will not bend easily, and will be therefore unmanageable, not only on first fixing it, when its being quickly drawn up is of much consequence to the success of the operation, but likewise in drawing it up tighter afterwards.

If it be too thin it will cut into the substance of the gland, which it should not do, as it may produce some hemorrhage unnecessarily, or produce another inconvenience, which will be noticed hereafter.

The surgeon need not feel any alarm at the violent strainings, and retchings, and flushings in the face which come on the moment the instrument is fixed; as he may rest assured that all these alarming appearances will very speedily subside, however violent or frightful they may appear at first.

The patient having become composed and quiet, the tube may be tied to the cap with a piece of string or tape if the patient is too young to be reasoned with, or if an adult, it may remain loose in the angle of the lips.

The next day the wire must be drawn tighter. One end of the wire being bent over the tube must remain as a fixed point to pull from ; and the tube being held firmly in the surgeon's left hand, with the other hand he must draw the wire up about a quarter of an inch, more or less, according to the diameter of the tumour. This must be repeated every day till the tumour drops off, which will generally be on the fifth day.

Each time the wire is tightened it produces a good deal of pain, particularly in the ear ; but this invariably subsides in fifteen or twenty minutes ; and of course, as mortification proceeds in the tonsil the pain decreases.

When the tumour is nearly off, if the patient is very young he should be watched while sleeping, as the tube becoming loose, might, possibly, produce some inconvenience in the throat ; but if it be fastened to the cap, nothing of any consequence can possibly occur. If an attendant is present, it likewise prevents the tumour being swallowed when it is detached during sleep. This frequently happens to children when left alone ; as from their having had two or three restless nights, they sleep so profoundly that they are not waked on the separation taking place.

This is of no consequence in itself ; but the

friends sometimes feel anxious and uneasy at the idea of the child having swallowed a putrid mass; and therefore, when it has happened, merely to satisfy them, I have generally given some purging medicine afterwards.

During the strangulation, it is almost unnecessary to observe that a good deal of feverishness is excited, and a considerable discharge of saliva and mucus is poured out, which teazes the patient by its viscosity and disagreeable odour. I have sometimes given an antimonial medicine to keep the skin moist, but I have never seen this absolutely necessary; and as to any erysipelatous inflammation coming on, as Mr. Want mentions in his publication, the *Medical and Physical Journal*, as a consequence of strangulating tonsils, I know nothing of it. Milk and water to wash the throat and mouth with, is the simplest and most efficacious gargle, or an infusion of roses with the sulphuric acid may be used, if an acidulous wash is preferred,

CASES OF REMOVAL OF THE TONSILS.

It will be unnecessary to detail more than one or two cases which have occurred to me.

A young woman, about 24 years of age, had been annoyed a long time by both tonsils being much enlarged. On the slightest cold she was attacked with severe inflammation of the throat. As she was in service, these frequent and severe

attacks interfered with her duties, exclusive of her suffering. She applied to many medical men without obtaining any relief; and as the parts became so much enlarged and so painful as materially to affect her health, she procured admission at different periods into two of the neighbouring hospitals. In one of these, gargles of various kinds were prescribed, and many kind of medicines. Receiving no relief from the gentlemen in that hospital, she procured admittance into another. Before she was admitted into the last hospital, I had seen her by desire of her mistress, and I assured her that the tumours could be very easily removed. She was, however, advised by her friends to try if she could procure relief in the hospital. The surgeons determined to try the effect of the application of caustics; and, as I learnt from the young woman, it was applied every morning, or every second morning, for several weeks. Slough after slough was thrown off, but without any diminution of the gland. In fact, the surface to which the caustic could be applied was so small, compared with the size of the tonsils, that it was absolutely impossible to destroy them by the caustic. It excited, as might be expected, much diseased action, and the girl, after having suffered this severe discipline for several weeks, I think six, left the hospital; her general health much impaired, the tonsils increased in size, and beginning to ulcerate. She immediately applied to me, and it became necessary to remove them immediately, as I had seen

enough of the chronic ulcerated state of diseased tonsils to determine that it should never be allowed to proceed from indecision or de ly on my part.

The operation was performed, as I have described, on the right tonsil, which fell off in six days; and afterwards on the left, which fell off in five. The girl soon recovered her health, and continues perfectly well.

Some short time after this young woman had recovered, Mrs. Mucklow, of Newhall-street, in this town, called upon me, and requested me to examine her grandson, a child about three years of age. The child was sallow and thin, and had the appearance of labouring under serious disease.

The account which I received was, that from his birth the child had always laboured under great difficulty in breathing, and particularly when he slept. That when sleeping, particularly if he turned on his back, there was such difficulty, such snoring and convulsive snorting, that it alarmed the whole house. The relations of the child were kept in such perpetual apprehension that the child would be suffocated, that a person constantly watched him whilst he slept, to change his posture when he became more than usually convulsed. From the immense efforts of the child to expand his lungs, and from the suffocative sensations which tormented him, his sleep was at short intervals and unrefreshing, and he always awoke in profuse perspirations from alarm and affright.

Nothing was known of the cause of all this distress. The openings of the nostrils, though remarkably small, were sufficiently large to admit air through them without causing such distress as the child experienced. Whilst I was considering what could be the cause of such difficulty, which, from the snorting that had been mentioned, I supposed to arise from some malformation of the cavity of the nose, I enquired if the child had any difficulty in swallowing; and on being answered in the affirmative, I of course examined the throat. Then the cause was evident enough. The uvula and tonsils formed a complete screen at the back of the tongue, beyond which nothing could be seen, and every atom of food or drop of fluid which passed into the stomach, must have been forced there by the greatest efforts of the organs of deglutition,

By removing the enlarged tonsils I was quite certain that the child would be restored to health, and all the convulsive snortings, &c. which had so long alarmed his parents and friends, would cease. But how this was to be effected with this little boy, who was too young to be reasoned with, and too old to be deceived, became a most serious consideration.

However, as this child could not have lived much longer if the glands were permitted to remain, and if they should increase in size, which was to be expected, I determined to attempt to remove them. I confess I entered on the operation with great anxiety, for it occurred to me,

that, even if I should succeed in fixing the wire (which was not quite certain) there was much to be feared from the previous alarm which the child might be thrown into by the preliminary preparation of gagging his mouth, which was indispensably necessary. In addition to this, the very severe pain and suffocative feel which would inevitably follow the strangulating so tender and irritable a part, caused me, even at the last moment, to doubt the possibility of succeeding in the operation. Exclusive of all this, and the difficulty to be apprehended by the unmanageableness of a child of this age, and there was much to be feared from this little boy, as he was one of the most shrewd little fellows that I had almost ever met with, there was to be superadded the inflammation that necessarily at first comes on, which would still more gorge up the passage, already too narrow to admit of respiration but with the greatest difficulty. However, as the child's friends were very anxious for something to be attempted, now that I had excited some hope in their minds, and being firmly persuaded that he must be very soon suffocated, if he could not be speedily relieved, I determined, at all events, to attempt it.

The child's hands, and body, and head, were of course firmly secured; his mouth was gagged, or propped open, by inserting a piece of soft wood between his teeth, and my son depressed his tongue with a spoon. I thus had a view, for the first time, of the extremity of the tonsils,

which were considerably more than an inch in length, of an oblong figure, and descending to the epiglottis. I fortunately slipped the noose over the left tonsil at the first attempt, and on perceiving it home, in the sea phrase, against the side of the fauces, and of course at the very base of the gland, I hastily tightened it, and fixed it there.

Then all which I foresaw as likely to happen occurred. The convulsive retching, and straining, and screaming of the child, was excessive and frightful. The tube was loosely tumbled about, hanging by the gland, with alarming agitation; for to have taken hold of it to have steadied it during this scene of convulsive exertion and agitation, would have been to have torn it away with the whole tonsil attached to it. Had it been possible to have taken hold of it with safety, I believe I should have removed the ligature, and have renounced all attempts of removing tumours of the throat from such young patients for the future.

This very alarming exertion brought on a pause from absolute exhaustion; and my son suggested to me to tie a piece of thin tape round the tube, and pin it loosely to the child's cap; by which means, if the same convulsive action should recur, the tube would be, in some degree, held steady. This was done, and it had the desired effect; the agitation soon ceased, and the child became calm and tranquil.

I never witnessed such a scene as this at any operation that I have been present at. On drawing the wire tighter the next day, I had an-

other repetition of the agitation, but by no means so violent, or of so long continuance. On each succeeding day there was some little restlessness and alarm excited, as my little patient was too young, as I have said, to reason with ; this prevented me drawing the wire sufficiently tight each time, and consequently prolonged the time of the gland falling off, and it was not detached till the sixth day.

The child was much relieved by the removal of one tonsil ; but the other was so large, and extended so low down in the larynx, that the breathing, particularly during sleep, was still very much impeded. The friends of the little boy were anxious, therefore, that the other should be removed. I consented to this ; but I was fearful that we should have a repetition of the former alarming scene. The child, however, was much firmer, and more composed ; and, comparatively speaking, there was not much to excite our fears. On tightening it daily, my little patient, with a degree of courageous firmness, which was surprising in such an infant, submitted quietly, and this tonsil therefore came off on the fifth day.

The child rapidly recovered ; his cheeks soon regained the flush of health, and his eyes the brilliancy of youth. He slept quietly, and breathed and swallowed with perfect ease. I think this case is rather interesting, as showing that the operation can be effectually and safely performed at any age, if an exigent case should occur ; as certainly there would not have been so much difficulty at an earlier or later period of life ;—at an

earlier, because a younger infant would not have had any other fear than what the pain produced; and at a later, because the reason might have been appealed to, and the resolution thus might have been strengthened.

I am quite certain that these glands could not have been removed by excision. If it had been attempted, and hemorrhagy had followed, the child must inevitably have died; as it would have been quite impossible, in such an infant, to have taken the necessary steps to restrain it.

I could give many other cases in illustration, were it necessary; but these two will be, I suppose, sufficient.

It may be necessary to mention, that in one case of a young lady, whose tonsils I removed, the wire was thinner than what I had used in general. The consequence was, that instead of gradually strangulating the gland, I cut into it. As I was aware of this, I tightened it very gradually. On the sixth day the tube came away, the wire being drawn close up to the end of it, but to my surprise the gland remained adhering to the fauces. This appeared very inexplicable, as the wire had passed completely through the substance of it. I was therefore under the necessity of applying it again; and the gland fell off in the course of the day.

Perhaps this may be thus explained. The wire having cut into the substance of the gland, the raw edges thus formed adhered before the central vessels were strangulated, and before the strangu-

lation was completed, new vessels probably were formed at the external edges; thus keeping up some circulation in the external part, while the wire was passing through the centre.

CASE OF A LARGE TUMOUR WHICH WAS REMOVED
FROM THE INTERNAL ANGLE OF THE JAWS.

The following case, of which a drawing is given, will tend to illustrate the preceding cases, as it was removed in the same manner as those were. Mr. William Robinson, of the Beech Lane, near this town, about sixty-five years of age, applied to me to procure some relief for a large tumour which almost completely filled his mouth. The account which he gave me was, that as long as he could remember there had been a very small swelling at the angle of the jaws, in the inside of his mouth. That about twelve years since, a hard snow-ball was thrown at him, which hit him on the outside of the cheek, immediately opposite the small tumour, with great violence, so great, that the pain which followed it compelled him to go to bed. From that period, he said, the tumour began to increase, till it had reached the size, which may be guessed at from the annexed engraving, and which is a very accurate representation of it when his mouth was opened.

On touching the tumour, I found it very firm; some parts were harder than others, and there

were some hard knots in the substance of it. It was slightly painful at times, and seemed to be in the first stage of schirrosity. As the tumour increased in size, the teeth were loosened by the pressure of it; and they had been removed, as they often hurt him when he was masticating his food; and frequently he bit the tumour in his sleep, as he was continually dreaming of eating, as he told me.

This is rather a curious fact, and somewhat tends to strengthen the ingenious speculations of philosophic men, that the act of dreaming takes place at the moment when the body, having regained, by sleep, a sufficient portion of excitability, is roused from its torpid state. Perhaps, in this case, the muscles which are employed in mastication, and particularly the buccinator, having been stimulated by that distention which is usually caused by food, the mind, in the half torpid state in which it is on waking, before it has recovered or regained its perfect thinking powers, associated the idea of mastication with that distension which the presence of this tumour caused; and before the mind was sufficiently roused to discriminate or ascertain the nature of this distending body, the jaws, from this association, were, perhaps, unconsciously put into action. I must beg pardon for thus wandering into the delusive, but most pleasing and seducing regions of theory. My readers will perceive that I do not often indulge in these excursions.

His tongue was pushed completely to the right side of his mouth, and consequently his articulation was very imperfect. On introducing my finger into his mouth, I found the tumour pyri-form in shape, evidently having its origin at the angle of the jaw, where there was a tendinous-like substance distinctly felt, surrounded by condensed cellular membrane, or steatomatous matter. From frequent injuries and inflammation, the root of it, which I suppose must have been at some former period very small, perhaps the tendinous substance which I felt forming a mere pedicle, had extended itself and became attached by adhesions being formed to the throat and tonsil of that side. As there were some vessels that beat strongly, and having once cut off a large tonsil that bled profusely and puzzled me a good deal, and as I could not get to these vessels to tie them with a needle or tenaculum, I was fearful of cutting this tumour away. I suppose that the fear of hemorrhagy had deterred other surgeons whom he had consulted from removing it, as all of them, he told me, agreed in the necessity of its removal, but they all declined taking it out. The person being anxious to get rid of this mass, which he expected would destroy him sooner or later, he was willing that I should remove it in what manner I thought proper.

I explained to him that I should not cut it out, that it was too large to be removed immediately, and that it would be five or six days

before it would come away. He therefore came into the town, that I might see him daily. He was desired to live low for a few days before he came, and to take some opening medicine.

The ligature was applied in the same way as is described in the preceding cases for removing tonsils; of course there was less difficulty in applying it to this large tumour. Almost immediately on the ligature being drawn tight, the tumour increased considerably and perceptibly in size, so much as to alarm the patient, who thought it would suffocate him. The tumour became also almost immediately of a bluish red colour, perhaps from the venal blood being impeded in its reflux; and its increase in size, I suppose, arose from the same cause, whilst the central arteries not being sufficiently strangulated to block up the passage through them, poured in their contents.

The tumour being so completely exposed to view, we saw the daily progress towards mortification. On the sixth day, the wire being drawn home, and the mass being completely sphacelated, I expected it to fall off. I expected it would certainly drop off the next day; but it still continued to adhere to the throat. The tube was now very loose, and could be twisted almost round. Another day elapsed, and it still continued in the same state. I examined the parts, and found that the tumour was retained merely by the tendinous or ligamentous substance, which I had felt on my first examination, in the

centre of its neck; this I divided with a pair of curved scissors (such as are used for cutting the frenum under the tongue of infants) and it fell off. This ligamentous substance seemed to have been a mass of condensed vessels.

After this person had got rid of this troublesome substance, which had pressed his tongue completely to the opposite side of his mouth, and, consequently, had elongated the muscles of it, he found, to his great surprise and dismay, that his tongue was not under the controul of volition, but tumbled about in his mouth, and, consequently, his articulation was worse than before. I assured him, that as the muscles gradually contracted, they would not long continue rebellious, and that he would soon regain the command over them, and consequently, that his speech would be as clear and distinct as it had been. The man was pleased to hear this, but I believe he did not credit me. This has, however, since taken place, to his very great joy.

On this principle, tumours may be removed from all the accessible cavities of the body, without much difficulty. The cavity of the nose must be excepted, as the difficulty of removing pendulous polipi from that part by ligature is very great, as I shall shew hereafter.

ON REMOVING TUMOURS FROM THE UTERUS AND VAGINA.

It is well known how difficult the removal of polypi from these parts has been found, even when it has been attempted by the most skilful men. As I have had some experience in this part of my profession, I have thought it might, perhaps, be of some service, were I to relate some of the cases that have occurred to me, and to describe the method of removing them which I have adopted.

Without affecting to have made any great discovery in the manner of removing tumours from these cavities, I think that the plan which I have adopted, and which I have found uniformly safe and effective, will be found more simple, and consequently less difficult in its application, than that which has been generally in use.

The difficulty of removing polypi from the uterus is, sometimes, almost insuperable. Even Doctor Denman tells us, in a very instructive case, which is related in the *Medical and Physical Journal*, that he was foiled, and was obliged to desist in attempting to apply a ligature upon a polypus of the uterus; and although he did ultimately succeed on a future day, it was with the greatest difficulty.

If a gentleman of Doctor Denman's acknowledged abilities and skill, and which have been solely devoted to the practice of that part of our profession, in which he is so eminently distin-

guished, and to which the management of these cases more particularly belongs, should acknowledge the great difficulty which he found in applying ligatures in the usual manner, it must be quite evident, that men with less information and little practice, must find it often impossible to succeed in their attempts.

In the Medical and Physical Journal for July, 1811, a case of polypus of the vagina is related by a gentleman of the name of Fordham, in which he says, Doctor Clough, a gentleman eminent in the practice of midwifery, passed a ligature round its stem, with some difficulty, although the extremity of this polypus easily passed through the os externum, and it arose from the posterior part of the vagina, having no connexion with the uterus.

In this case, which Doctor Clough found some difficulty in passing the ligature round, I do believe, by the simple method which I shall hereafter describe, that it would have been strangulated with the most perfect ease; perhaps I shall not exaggerate, if I say that it might have been passed round it almost as easily as a ring could be pushed on a finger.

In the same Journal, for October, 1814, a description is given of what is called an improved instrument for the removal of polypus uteri. It is given from the German by Mr. Want. I must refer my readers to that work for a description of it. It certainly is no improvement.

In various books there are descriptions given

of instruments which have been invented for this purpose. The misfortune is, they are all too ingenious; they are too complex. In such confined parts as the uterus and vagina, it is with great difficulty that the simplest instruments can be applied, as every one knows, who has attempted it, how difficult it is to apply the noose round the body which is to be strangulated.

The great desideratum is, to procure an instrument that shall, with tolerable facility, convey a ligature to the origin of the tumour, to its very base. And having found a simple instrument to effect this, the next important consideration is, to procure a ligature made of such a substance as shall be sufficiently flexible to be easily opened with the fingers, or with a finger in the cavity, so as to be passed over the globular body of the polypus; and yet, immersed as it must be in the natural discharges of the part, shall retain sufficient firmness, that is, shall not become soft by the absorption of moisture, and thus ravel up, and be rendered incapable of sliding smoothly over the surface of the tumour which is to be strangulated. With a proper substance to form the ligature of, the old instrument for strangulating polypi with, would answer the purpose; but I think a tube, or rather a double tube, is preferable. If the ligature combine the qualities which I have described, it will not be of much consequence whether the surface of the polypus be smooth or rough with excrescences, or irregular with tumours; as by the aid of a rod of any

metal, with a hole pierced near one end of it, the ligature may be lifted up, and passed over any irregularity or protuberance.*

To remove tumours from the uterus or vagina, all that is wanted is a single or double tube, and a rod of any kind of metal of equal length. The tube should be about ten inches long for removing polypi from the fundus uteri, and proportionately long for such as are attached to the cervix uteri and vagina. To remove those from the

* I believe it does not often happen that there are excrescences, or irregular knobs on the surface of polypi of the uterus. It is almost impossible that irregularities should grow on polypi of that organ. All that I have examined, as far as I could feel, have been smooth, and I should apprehend, in the nature of things, that this smoothness was general over the whole of their surface. The muscular pressure is so immense, as is well known to practitioners in midwifery, that I can scarcely conceive it possible that there can often be much irregularity on the surface of these tumours. It is not easy to know what the surface of a polypus has been previous to its extraction, excepting as far as it can be felt by the finger, as before it comes away after strangulation, it is in a putrid state. A curious proof of the power of the uterus was evinced in a case which once occurred to me. In attending a lady of this town in labour, a child was very rapidly expelled, and on taking hold of the funis to assist in the delivery of the placenta, on a little pain coming on, I found something protrude, but of what nature I could not ascertain. A sharp pain soon expelled a fœtus about the length of a child of about the fifth month, squeezed perfectly flat. It so happened that the fœtus was flattened in such a direction as to leave the profile complete. I suppose during the gestation of this female, by some accident or other the membranes were broken, and the water discharged; and thus the child being unprotected by a surrounding fluid, was exposed to the whole contractile power of the uterus, which had thus flattened it. I could not learn from the lady that she had ever perceived any fluid escape. Is it possible that the liquor amnii could have been absorbed by any means? I have read in some of the journals of a somewhat similar case to this.

fundus uteri, the tube or rod of metal should have a degree of curvature as is shewn in the plate. The rod of metal must have a hole pierced through one end. The curvature of the tube is necessary, as all polypi of the uterus, when arising from the fundus, must be nearly globular, from the necessary pressure of that organ; and therefore the concavity of the tube being adapted to the convexity of the tumour, lies easier, and creates less distension, and consequent irritation, than the usual straight instrument.

Through this double tube is to be inserted a strong piece of gimp, which is used in pike-fishing, or the bass string of a bass viol, either of which are admirably adapted for the purpose. Either of these are to be used as the silver wire is used for strangulating tonsils; but the ligature is to be inserted through the eye in the metal rod before it is inserted through the second tube, as is represented in the plate.

The rod and tube being curved precisely alike, are to be placed together, and the ligature being drawn so as to form a noose sufficiently large to pass on the fore finger, is to be introduced on the end of the finger of the left hand into the mouth of the uterus, while the tube and rod are pushed up with the right hand in contact with the left finger. On touching the polypus where it presents, resembling, if large, the head of a child, the noose is to be gradually opened by the finger in the uterus, spreading it out in a circular direction till it is sufficiently opened to pass the presenting

globular polypus. From the elastic flexibility of the bass viol string or gimp, this is done with inconceivable facility, however narrow the part, and whatever may be the pressure. When the noose is thus sufficiently spread, the tube and rod are to be gently introduced higher, and the ligature is to be pushed up round the polypus, as far as the finger can reach. Having done this, the finger is to be withdrawn, as it can be of no farther use. The tube and rod are, with one hand, to be gently pushed up, whilst the ends of the ligature are to be drawn slightly tighter, so as to embrace the polypus, and being carried up by the tube and rod, it will gradually slide up the polypus, on its surface, between it and the walls of the uterus. This is done so easily as cannot be conceived by those who have experienced the difficulty there is in introducing the common instrument with a cord, or a skein of silk or thread, which soon becoming moist with the discharge, hitches, and cannot be moved up without the assistance of another instrument, and very frequently, as I have heard of, not with the aid of that. The difficulty likewise of spreading it out, in the first instance, is almost insuperable, from the same cause of the ligature absorbing moisture, and becoming loose, and not being elastic.

After the ligature and tube are carried to the top, the former must be drawn tolerably tight, and the ends of it are to be fastened to the extremity of the tube. The tube and metal rod remain in contact, and will create no uneasiness.

This is the manner in which a smooth polypus may be strangulated, one on the surface of which there are no knobs nor protuberances. Where it happens that there is irregularity on the surface of the polypi, which in the nature of things cannot often be, if we recollect the *prodigious contractile force of the uterus*, and which is so well known to gentlemen who practice midwifery, when it becomes necessary to turn the child, after the water has been evacuated : when, therefore, there are irregularities on the surface, if they are of any size, it is evident that the ligature cannot slide over the surface smoothly, as I have described it. It then becomes necessary to use the rod of metal, which I have before mentioned (and which it is only necessary to use in case of irregularities) to lift the ligature over the protuberances wherever, or however many they may be, and to pass it up to the base of the tumour, in aid of, and in correspondence with, the motion of the tube.

By the assistance of this rod of metal, *this kind of ligature* can be passed over any sized protuberance with the greatest ease. The rod occupies so little space, and being curved in the same degree with the tube, whether it be necessary to use it or not (which cannot be ascertained till the attempt to pass the ligature is made, as the finger can seldom be introduced high enough to ascertain this) it should be introduced always with the tube ; and after being used, if necessary, it is

brought round to the tube, and remains in contact with it, till the tumour is detached and brought away.

The ligature must be drawn tighter every day according to circumstances (always recollecting, if great pain and tension of the abdomen come on, and all those symptoms which denote beginning inflammation of the uterus, that the ligature must be immediately loosened) and the separation will take place from the sixth to the tenth day, according to the size of the polypus.

Where polypi are to be removed from the vagina or cervix uteri, the tube and rod may be straight, as there is not in general much convexity in polypi of either of those parts. The difficulty of strangulating them will not be so great as where the tumour is attached higher up; and in those of the vagina, which appear externally, if the part which appears without the os externum be taken hold of with a pair of forceps, or if a wire be passed through it, to hold it firm, the silver wire, or the bass viol string, may be passed over them up to their base with the greatest facility.

I can with great confidence recommend this method of removing polypi from the uterus to medical men. I have, on this principle, succeeded in removing tumours of this kind when attempts have been made without success by very able men. It is not a want of knowledge of the parts, nor a want of adroitness, that causes

these failures, but the want of a proper apparatus. Every person who has attempted to introduce a ligature formed of waxed threads, or of cord, or of any of the substances which are usually used, knows what a clumsy business it is to get it up, perhaps after attempting it for more than a hour; and it is well known, that women have died miserably, from it having been found impossible to remove polypi. I do believe that almost all kind of polypi may be removed by this plan. The size is of no consequence, as the ligature may as easily be applied upon the largest as upon a small one, the contraction of the uterus being, perhaps, greatest where it is least distended.

After the ligature is effectually applied, it becomes the duty of the physician or surgeon who has the care of the patient, to watch the effect which it may produce on the general health. It must be recollected that the polypus arises from, and is identified with, the uterus, and that a very small injury inflicted on that important organ often produces fatal consequences. It must also be recollected, that what has been called symptoms of irritation, often come on after great operations, or when organs of great irritability are affected, before inflammatory action has scarcely commenced. If, therefore, any of those symptoms occur, the ligature must be immediately loosened, and it must not again be drawn tight till sometime after all those symptoms have ceased.

These symptoms may be expected to appear,

if they do appear, in twelve or eighteen hours after the strangulation is made. If, however, there be no appearance of these symptoms in the early hours after the operation, we must not forget that inflammation of the uterus may possibly arise, and, therefore, great attention must be paid to the state of the pulse, and to the feel of the abdomen, and generally to the state of the whole system. If symptoms of inflammation come on, and if bleeding, and cold applications, and purging, and other means of depletion do not arrest them, the ligature must certainly be relaxed; and if they appear again on its being tightened, it will be advisable to postpone the operation to a future period. It must be evident, that the greater the lapse of time after the operation before any alarming symptoms appear, the less must be the danger, as every hour that passes is tending to produce death in the part, and, consequently, separation from the uterus.

The following case, which I have selected from others because it has occurred very lately, and in which the polypus originated by a broad base from the very fundus uteri, will, perhaps, sufficiently illustrate what I have written.

An unmarried woman, about forty-four years of age, had been ill for nine or ten years. She had been tormented during that whole time, more or less, with stretching pains about the region of the uterus and bladder, which extended to the hips, and about the whole belly. At the time of menstruation these pains were much

increased, and the menses were occasionally excessive in quantity. For the last two years her sufferings were considerably greater. A constant discharge issued from the vagina, and at short intervals she had much uterine hemorrhagy. These discharges, and the increasing pain which she suffered, had reduced her extremely. Her countenance was very sallow, with frequent hectic flushings, her pulse was from 110 to 120 in a minute, her nights restless and without sleep, and her appetite almost gone.

The disease, I apprehend, had been treated as schirrous or cancer of the uterus, and she had, as is often the case, in such a miserable state, wandered about to medical and surgical practitioners of all kinds, regular and irregular, in search of relief.

When I saw her, I was fearful that nothing could save her life, but as the symptoms clearly pointed out that some serious disease was situated in the uterus, and as, from the whole of the case, I was disposed to think it was polypus, I requested permission to examine per vaginam.

I found the os uteri open about the size of a shilling, and found a polypus presenting, feeling precisely like the occiput of a child, and about the same size. With great ease I gently and gradually dilated the mouth of the uterus, and introduced my finger as far as I could reach round this almost globular and smooth body. The tumour was firm, and bore considerable pressure of my finger without producing any pain.

Having perfectly ascertained the cause of the disease, it became necessary to remove it without delay, as the person's health was so much reduced, as to make it necessary to do, what could be done, quickly. I had, however, very great doubts if it were possible to save her life by removing the tumour, she was so much reduced. I, therefore, desired to consult with a physician as to the propriety of the operation, and Doctor John Johnstone saw her with me. That gentleman agreed with me in the necessity there was for removing the polypus, as the only chance of saving the person's life.

As I had no tube long enough to reach the basis of the tumour, which I had ascertained was attached to the fundus uteri, and as the person was very impatient to have the operation performed, now that some hopes of relief had been held out to her, I made use of a curved piece of metal about as thick as a quill, with an opening at one end of it. Through this opening a strong piece of gimp was introduced double, which loop I carried on the point of my left finger, through the vagina, and past the os uteri, which I had gradually dilated to enable me to do so. When the noose was fairly carried by my finger into the uterus, I gradually introduced the instrument, guided by my hand and by the finger in utero, and pushed it up gently with my right hand. I then dilated the noose till it formed a circle round the bottom of the polypus, and pushing it up as high round the tumour as I

could, I pushed the curved piece of metal up with my right hand, and desiring my son, who assisted me, to gently tighten the ligature, it slid upon the surface of the tumour, gently embracing it, up to the very origin. The ligature was drawn tight, and fastened to the piece of metal by a projection which was fixed on it for that purpose.

Although the base of the tumour was very great, which was ascertained by the large portion of the ligature which was occupied in passing round it, no very great pain was produced by drawing the ligature pretty tight. The operation was performed about four o'clock, and at night my patient was as comfortable as before it was done. I gave her an opiate, which procured some rest.

On the succeeding day the ligature was drawn tighter, and it produced a good deal of pain, but it gradually subsided. On the third day her pulse became quicker and harder, there was some fulness and tenderness of the belly, and frequent flushings in the face; but on the whole the symptoms were not very alarming, as something of this kind was to be expected. Her bowels were kept open, she lived on a strict vegetable diet, and drank only water; she had an antimonial medicine and an opiate at night, which procured her some sleep. The tension of the abdomen was not so great as to induce me to bleed her, which I should not have had recourse to but from very urgent symptoms, as she was extremely

weak and reduced. After this she went on tolerably well till the seventh day, the ligature being tightened daily, when very great restlessness and heat came on. The discharge was in great quantities and very fetid, and on examination I found the tumour descending through the os uteri. The vagina was washed out with warm water by injecting into it with a syringe very often. On the night of the seventh day, the restlessness, which had been coming on the whole day, considerably increased, her face was very much flushed, and her pulse were at 130. There was not, however, any increased tension of the belly, nor was there much pain in the uterus. She could likewise bear moderate pressure on the abdomen without pain.

As the time was past when inflammation of the uterus was to be expected, I did not bleed her; in fact, it puzzled me to account for this great restlessness and heat, as the polypus was nearly separated from the uterus, as was evident from the profuse fœtid discharge, and from the tumour having descended considerably into the vagina. I ordered her glysters, the vagina was kept as clean as it was possible; she had decoction of bark every three hours, and took her opiate at night. The restlessness still continued, and almost produced delirium, and she complained of great soreness and itching about the labiæ and nymphæ. On examining those parts, I found that all this mischief arose from the ligature pressing upon the external opening of the vagina,

which had fretted and irritated the skin to a great degree, and which was very hot and inflamed. This was the consequence of using an instrument that was not quite adapted to the purpose. Had a double tube been used, nothing of this sort could have occurred, as the ligature would of course have been covered by the tube, and would not have caused the least uneasiness.

The restlessness caused by this irritation was very distressing during the whole of the ninth day, and no guarding of the parts, which I could devise, allayed it. The discharge, which was very fetid and acrid, added to this, although all possible precaution was taken to wash it out with warm water, decoction of bark, and some mucilaginous fluid. On the evening of the ninth day, a considerable portion of the tumour came away in a putrid state, and, I believe, with a little force, I could have removed the whole, as the ligature appeared to be drawn quite close to the instrument, or in the sea phrase, "quite home." I thought it better not to use any force, as it was quite evident, that in a few hours it would necessarily fall away of itself. On the tenth morning this did take place, the instrument, with the remaining portion of it, of great size, was excluded by a little uterine contraction.

It is unnecessary to describe the comfort my patient felt from the removal of this tumour. She gradually recovered her health, and although the last winter has been severe, and she only under-

went the operation in October last, she is now, March the third, perfectly well.

I believe the greatest pain and inconvenience which my patient experienced arose from the fretting of the ligature, which could not possibly have taken place, had the double tube been used, as a mere piece of silver would have lain in the parts without producing the least irritation.

In this case the instrument which is commonly used, and which is quite straight, could not have been used with effect, as the body to be removed was almost globular. If, therefore, it had been applied so tight as to effectually strangle the base, the mouth of the uterus must have been dangerously dilated by the necessary propulsion outwards of the shaft of the instrument. And although no danger had arisen from the great dilatation of the os uteri, many more days must have elapsed before the separation could have been effected, or before such strangulation could have taken place at the root of the tumour as to have produced death in it; thus prolonging the chance of inflammation arising in the body of the uterus, as its impression on the basis would have been sufficient to have excited increased action, but not sufficient to have destroyed the vitality of the part.

I again repeat, that I can recommend this instrument, and this kind of ligature, with the greatest confidence to medical men, as I am certain, if once tried, they will never attempt to use the old instrument, and the thread or cord as a

ligature. The gimp, or bass viol string, are the best possible substances that can be used, as they combine flexibility with elasticity and firmness, and being formed, the one with catgut covered with a metallic wire, and the other with silk, covered with the same, they are not softened by absorbing moisture, and therefore they will slide up between the surface of the tumour and the sides of the uterus with the greatest facility, and thus will be simplified an operation at all times difficult, from the narrow and contracted parts in which we are to operate.

I could give other cases of this kind, but to relate them would be only to occupy the time of my readers unnecessarily, as the relation of the foregoing gives a tolerably accurate view of what may be expected in similar cases.

ON REMOVAL OF TUMOURS FROM THE RECTUM.

The removal of tumours from the rectum, as far as I know of, is seldom, if ever, attempted, without they are situate near the verge of the anus, and can be forced down by the expulsion of the fæces. Mr. Hey has given some excellent practical remarks on this subject; and indeed, on whatever matter he touches, his remarks are always excellent.

There is not, however, any thing peculiar in the rectum to prevent the removal of tumours from that part, and the mere narrowness of the parts is not sufficient cause to prevent the sur-

geon from operating when necessary, as that difficulty is not insuperable.

From what I have seen, I apprehend that the presence of tumours in the rectum is more frequent than is generally supposed. I know that they have deceived many very accurate observers, and the symptoms which they have caused have been fatally mistaken for other diseases.

Unfortunately they do not produce any precise diagnosticks, and therefore they are often confounded with schirrous contraction of the rectum, and with other diseases of that bowel. I should recommend to surgeons to accurately examine the rectum in doubtful cases, and not to trust to one examination; as I have not been able to discover the tumour at all times, when I have known, from previous examination, that there has been one.

A person applied to me who had been long tormented with distressing pains in the rectum, the pains darting up in all directions, and by sympathy, often affecting the bladder, and producing straining and an unceasing desire to expel the urine. There was a good deal of blood occasionally discharged by stool. For a year before I saw him, the pain had been almost unceasing. He described it as a dreadful burning pain, shooting into the bladder, and occupying the whole region of the perinæum and anus, and frequently darting into the back, and about the sacrum. Whenever hemorrhagy came on, he was relieved for some days; but the pains soon returned, and gave

him such short intervals of quiet that he became emaciated and sallow, he lost his appetite, night sweats came on, and all the train of symptoms that accompany long continued severe pain.

He had had the operation for fistula in ano performed about two years and a half before, of which he was quite well. I did not examine per anum when I first saw him, but as no relief was procured by what I ordered, which were glysters of various kinds and gentle aperients, with the occasional use of opiates when the pain was violent, I introduced my finger, and I expected and feared that I should find a schirrous and contracted bowel. As that was not the case, I carefully examined the prostrate; but that and the bladder appeared in quite a natural state. Before this I had introduced a sound into the bladder, but without gaining any information. I perceived high up, however, a hard substance, but it was at such a distance that I could neither ascertain its shape nor of what kind it was. It appeared to be the cause of the disease.

I wished to consult with the surgeon who attended him before; but he objected to this, as his former surgeon was very intimately connected with him, and he thought he might feel offence at his having consulted another person. I entreated him to live very low, to carefully avoid all stimulating food, and to keep his bowels open. I ordered him the cicuta, under the idea that the disease was cancerous. It failed to do any good, as in cancerous cases it has always failed, as far

as my experience goes. The extract of belladonna and hyoscyamus were given by the mouth, and sometimes were injected into the rectum, and opium was given when the pain was overwhelming. This gentleman had suffered a good deal from lues, and I therefore was induced to try what mercury would do. It certainly did injury, and was, therefore, immediately left off. After sometime I again examined the rectum. The tumour had descended much lower, and I could now ascertain that it was a long substance, rather hard, and apparently about half an inch in diameter, and it gave him some pain on being touched. With a long probe, I could trace it high up in the rectum.

As before this time I had become acquainted with the method of removing tumours from other cavities, I thought it was possible to get hold of this substance; and it was quite evident, that if such tumours could be removed from the uterus with safety, there could not be any thing in the structure of the rectum to prevent my attempting to remove this tumour from thence. But in this deplorable case, if I succeeded in removing the tumour, I could scarcely hope to save the life of the man, as from all the symptoms, I could not expect that the disease was confined to the tumour; it was to be feared and expected, that the intestine partook of the disease, which appeared to be schirrous or cancerous. If this proved to be the case the removal of it would do little. I fairly stated to my patient my hopes, and as

he was a sensible man, all my fears, and left him to determine what should be done, as I had no experience to guide me. He determined that I should make the attempt.

He was of course kept low for some days, and had some opening medicine, and on the morning of the operation a glyster was thrown into the bowel, to wash away any fæces that might be lodged there. The first thing to be done was to fix the tumour, as it receded on being touched; and therefore, till that was done it was impossible to push up a loop of wire, or catgut, to strangulate it. To effect this I used a piece of strong silver wire, bent at one end almost double; this end was filed sharp, as with it I intended to take hold of the lower part of the tumour, by piercing it with the bent part, or hook, of this wire.

I passed my finger into the rectum with this hooked wire by the side of it, and easily succeeded in taking firm hold of the bottom of the tumour, by passing the hook through it. This hook I then pressed close to the wire, of which it was a part, lest its sharp point should injure the intestine. I withdrew my finger, and placing the loop of another silver wire on it, the two ends of which were passed through a double tube, as is explained for removing tonsils, I passed it up into the rectum on my finger, and carried the double tube up by the side of it. The tumour being held quite firm by the means which I have described, it could not recede; and the loop being disengaged from my finger, was pushed up by the tube

to the very origin of the tumour. It was then strangulated by drawing the wire tight. The pain was excessive, but in about twenty minutes gradually subsided. I gave him an opiate, and he had some sleep.

The pain was very great when I tightened the wire next day, but gradually subsided as before. The pain became less every day, and on the sixth day the tumour was separated and expelled. There was a good deal of tenderness in the part for some time afterwards, but there were no more of the lancinating and darting pains which had tormented him, and to my great surprise he gradually recovered.

This was a termination, which, under all the circumstances, could scarcely be expected, and I by no means wish to be understood that the operation can do any more than remove the tumour. Had the bowel partaken of the disease, the mere removal of the tumour, of course, could have done little.

It perhaps may induce any surgeon who may read this paper, and who may meet with a doubtful case of disease in the rectum, to examine more than once into the cause of the complaint; and if a tumour should be met with, by following the method which I have described, it will certainly enable him to remove it; as he will find that, comparatively speaking, it is not difficult to apply a ligature, by the aid of this simple apparatus, to the very base of a tumour, although it may be attached very high up in the intestine. It may be

necessary to repeat, that it is essential to the success of the operation, that the tumour be first fixed, by passing a wire, as I have mentioned, through the bottom of it; or if that cannot be done, a small pair of forceps, which will hold firmly, may be passed into the rectum, and the tumour may be thus held firm. The wire will, however, I know, answer. The possibility of using the forceps I only mention from a conviction that they may be used, as I have never tried them.

ON REMOVING POLYPI FROM THE NOSE.

Mr. John Bell tells us in his admirable work on Polypi, that it is not possible to strangulate those pendulous polypi that are met with in recent cases of the disease. He says he has been himself foiled, and has seen others likewise foiled in their attempts to fix ligatures, from the polypi receding, and from the opening of the bones of the nose being so narrow as not to admit of the tube and wire being pushed up high enough.

I know that Mr. Bell is perfectly right in what he has written on this subject; it is quite impossible to fix a ligature on such polypi. It is, however, their want of solidity and substance that prevents the ligature being fixed on them, as it is a very simple affair to fix polypi in their most pendulous and flexible state, so as to push a thin wire up them. But from their tenuity and want of firmness, being little more at first than mere vis-

eous slimy membranes, it is impossible to fix any kind of ligature upon them, be it thin wire or gimp, or any other substance. I fairly tried this a few days since in the presence of a medical gentleman and his assistant, and my son, on polypi of this kind, in the nose of a lady from Sutton Coldfield. In this case I very easily fixed the polypi by holding them firm with small forceps, and slid the noose up them ; but on pulling the wire tight it invariably slid down almost to the bottom of the polypus, there being no substance sufficiently firm to retain it. I removed them with the knife, on Mr. J. Bell's plan; but the same want of firmness prevented me cutting them off high enough, as I cut them off, or broke through them, before the knife was conveyed to their base, there not being substance enough to guide the knife. I tried to tear one away with a pair of thin forceps, but it was so tender as to break off near the forceps.

I believe that it is possible to strangulate pendulous polypi, if there be sufficient substance in them. The polypus must be taken firm hold of by a pair of forceps, or a small hook may be pushed through the bottom of it, and the noose of the wire may be slid over the forceps or hook, and may be pushed up the polypus ; when the tube reaches the base, the wire may be tightened, and the polypus strangulated. If the wire is too inflexible to pass up readily, the ligature may be formed with gimp, as the fishermen call it, which is flexible, and will not be softened by moisture.

When polypi are large and fall back into the throat, they are very easily noosed with the silver wire and double tube introduced through the nostril. I have removed many of that kind. I removed a very large one of that kind several years since, with the assistance of Mr. Mole, surgeon, Weymouth, who was then a pupil of mine. I accidentally met the person a few days since, and I was happy to find it had not returned.

It will not be necessary to mention how the ligature is to be applied in such cases, as this method has been described by several surgeons.

I have detailed the preceding cases in the hope that entering into the particulars of the mode of applying the ligature and the treatment, &c. may be of some service to the younger part of the profession. The first case of polypus of the uterus that I ever met with puzzled me very much, as I could not find any case in my books where the particulars were mentioned. The general description in systems of surgery are of little service, but as giving a general view of the subject, and the cases which I have seen reported in the periodical publications, merely relate the fact of the removal of the polypus.

In describing polypi of the uterus, I think what I have written conveys too much the idea of these tumours being quite spherical. I beg to mention, that I only mean, that, from the form and action of the uterus, polypi arising from its internal surface, must necessarily be in some degree globular.

Some Observations on Injuries of the Head.

I Scarcely know any part of surgery in which there has been so important a change in the opinions of surgeons and in their practice, within the last thirty years, as in what relates to the operation of trepanning for injuries to the skull.

This operation was certainly had recourse to by many of the surgeons of that time, who were deservedly held in high estimation, in such cases as the surgeons of the present day would not think themselves justified in performing it. I believe that some very eminent men scarcely considered the mere removal of a piece of bone from the skull as in itself dangerous. I perfectly recollect a surgeon of distinguished reputation, very deliberately declare, that he had never seen a case where he could fairly attribute fatal consequences merely to the operation. These opinions influenced the general practice for many years.

A very different opinion has of late years been making its way among surgeons, and there can be no doubt that the practice has been much amended by it. It certainly was very bad practice to apply the trephine in every slight fracture and depression. I have seen this done in many very trifling cases, where there has been no alarming symptoms present. Whether the present doctrine of abstaining from operating, but in the

worst cases, be not carried too far, seems rather doubtful. It has been stated by two justly celebrated men, that even very considerable depressions and fractures of the skull, when attended by all the symptoms of pressure on the brain, and even where, as it would appear from symptoms, inflammation had commenced in that vital part from that pressure, have been cured merely by depletion. Several very extraordinary cases are related by both these gentlemen to prove this, and very strong deductions are formed from those cases against the operation of trepanning, except in the most formidable injuries of the skull.

I know that many practical men think with me, that the opinions which I have alluded to are carried too far, and may lead to very serious consequences. The gentlemen to whom I allude have had such extensive practice, their judgments have been so matured by experience and observation, that numberless minute circumstances which would escape the observation of the young surgeon, would be perceived and accurately estimated by them, and would enable them, perhaps, to conduct the suffering patient, with the skull driven, as we are told, a quarter of an inch from its situation, down on the dura mater, raving and convulsed, by the lenient means of depletion only, to reason and to health.

I am almost certain that such terminations are not to be generally expected when conducted even by the most able men.

I perfectly well know that it is a very serious

affair to saw out a piece of the cranium and expose the dura mater; and I am quite aware of the many bad consequences which sometimes follow that operation. But, because such consequences sometimes follow this operation, are we to be deterred from removing a piece of this bone, when we have a patient suffering all the injuries which pressure on the brain produces, and which depletion will not in a short time relieve?

There must be infinitely more evidence of the safety of such practice than hath hitherto been produced before I can trust to such assertions. I confide in the facts recorded by these two gentlemen most implicitly. I have the very highest respect for their talents; but it does appear to me that they generalise too much, that they draw inferences which their facts do not warrant, and that they attempt to lay down general rules from a few extraordinary and almost miraculous cases, in opposition to the opinions and experience of almost all practical men.

The observations of Dessault are highly deserving of notice, and have no doubt had great influence, as they ought to have, on the practice in general in fractures of the skull, and more particularly on the practice in the Hotel Dieu. But if his example is to be followed to its full extent, surgeons must lay aside their instruments altogether, as he seldom performed that operation, at least in that hospital, so uniformly unfortunate his operations had been. The Hotel Dieu was always dreadfully crowded, and in such an im-

pure atmosphere, it could not be expected that persons who had undergone such an operation would do as well as others who had breathed purer air. We very well know that this operation is not so fatal in the most crowded hospitals in London. I have seen many fortunate cases in some of those hospitals, even when it was the practice to take away the whole of the scalp from over the fractured part, and when the hospitals were in a very filthy state, compared with what they now are. Dessault's practice in the Hotel Dieu cannot, I think, be quoted as a general example, though it is very necessary to be recollected, as it may prevent rash operations. I am told by gentlemen who have been lately in France, that it has not influenced the practice of the French surgeons so much as might be expected. In England, the operation is less frequently resorted to than it was thirty years since, as I believe most men are convinced that it is bad and dangerous practice to have recourse to it but on exigent occasions; and I think that neither the reasoning of one gentleman, nor the strong language of the other, will induce surgeons to trust to general remedies only, when the scull is badly fractured and driven in upon the brain, and where delirium and convulsion are the consequence, and where the various methods of depletion have been tried in vain.

I have stated here what I know is the opinion of many learned and scientific surgeons. For my own part, undeterred by the ludicrous lan-

guage which one of these gentlemen so well knows how to apply, to ridicule and overwhelm, sometimes I think with words only, those persons who differ from him in opinion, I shall proceed to state what has been my practice, and, maugre all the strong epithets which may be applied to it (if this humble work should be ever noticed by him) what will be my future practice, till something more convincing than any thing which I have yet met with induces me to alter it.

It has been my invariable practice in all injuries of the skull, either when depressed or fractured, if they are not so great as decisively and evidently to require an operation, to depend on bleeding largely and repeatedly, if the symptoms require it; to purge briskly, and to enjoin the strictest abstinence from all fermented or strong liquors, and animal food; and to insist on perfect quietness.

If these means should not relieve the patient, but, on the contrary, if the symptoms should increase and become more threatening, I certainly should not feel that I had done my duty, were I to omit stating to the friends of the person, that an operation appeared to be necessary. It would not satisfy my conscience, if a person died under such circumstances, for me to recollect the relation of an extraordinary case of a boy lying for days or weeks in stupor and delirium, and at last recovering without the trephine being applied; or of another where the bone was driven down on the dura mater, for more than a quarter

of an inch ; and that after the surgeon had *rudely laboured to heave* it up, &c. &c. without success, the little boy recovered with this piece of scull remaining pushed against the brain. I say this would not satisfy me. I should not dare to expect that miracles were to occur in my favour. I certainly would in such cases remove the depressed bone, *by rudely heaving it up* (so it may be called, if any gentleman likes the words); and if I omitted to do this, and the person died, I should think myself accessory to his death. Nor should I think, having thus *rudely heaved* up the depressed bone, if the person recovered, that I had been doing nothing towards that recovery.

The putting strong and extreme cases does certainly exemplify the meaning of a writer, and enables him to shew his powers of enforcing his opinions ; but it is going too far to draw general rules from them ; and yet it does appear that such rules, or principles as they are called, are attempted to be established from such cases.

When a celebrated writer on midwifery tells us that under certain circumstances, when the shoulder presents, the child will, by some strange action of the uterus be gradually moved round from that situation, and be expelled by its contractile power, without the aid of the accoucheur, he does not tell us, that we are always, in such presentations, to wait for this extraordinary evolution.

There are other cases likewise of injury to the head, where there are no depressions nor frac-

tures, that, in my opinion, and in the opinion of many other practical men, make it necessary to remove a piece of the skull.

In blows or falls, where no depression nor fracture are produced, it sometimes happens, as is well known, that the very spot can be accurately traced and defined where the injury has been inflicted. From such causes all those symptoms that denote injury of the brain sometimes follow; and if relief be not by some means or other afforded, death takes place. Now, in such a case as this, if all the modes of depletion have been had recourse to without affording relief, and if the patient from head-ache, and restlessness and sickness, becomes affected with stupor, and then with muttering delirium, and then with raving and convulsions; are we to remain passive spectators of this sad scene, and because some marvellous tales are told by this or that great man, that no pressure of a clot of blood on the brain can produce injury, that we are to wait and see this person die? Are we to be told that it is *rude and clumsy, and making surgery indeed of it*, if we apply a trephine and remove a piece of bone from this spot, so marked and defined? Such cases have happened to me, and where I have, as I have supposed, happily relieved my patients. I expect, however, I shall be told, that I had assumed to myself the merit which I was not entitled to, and that, if I had not been busy with my operation, and rudely boring out bone that never ought to have been interfered with,

my patients would have done quite as well; better many of them could not have done, because they recovered.

Assertions and conjectures are easily made; but what has happened cannot be reasoned away. A case has lately occurred to me, which I shall relate hereafter, that will illustrate my opinions; and, fortunately, I have been made acquainted with the outlines of another case, which a gentleman lately attended in this neighbourhood, and which will still further elucidate and confirm what I have written. As this case is, I understand, to be published, I shall only give a sketch of it, which I have learnt in conversation, and which I understand is correct. These two cases, one of which terminated favourably and the other fatally, will pretty clearly prove, that even where no injury to the skull can be perceived, neither depression, nor fissure, nor fracture, nor detachment of the pericranium, that in such a case, effusion of blood may take place under the bone forming a clot, and that such pressure may be made on the brain as, in one instance, to produce most alarming symptoms, which were only relieved by removing a piece of bone; and in the other, did actually produce death.

When these cases are related, I hope it will shield me from the imputation of being a busy meddling being, always ready with my trephines and heaven knows what besides, to bore here and there in a quixotic search after fissures and depressions round the globe of the head. I am

as deeply impressed as any man can possibly be with alarm at unnecessarily applying the trephine; but I must firmly state my opinion to be, that where injury has been done to the head, without producing either fracture or depression, if the symptoms become urgent and continue to increase, although purging, and repeated bleedings have been resorted to, and if the part injured can be accurately defined, then I do positively affirm, that in my opinion a trephine should be applied to that spot to remove the bone. By doing this, I believe in many instances a clot of blood will be found, and by removing the bone the person will be relieved. I must beg that I may not be understood as recommending this to be done in all cases of concussion, as I very well know that in many such cases it would be highly improper, the cause being deep seated and far beyond the reach of the trephine.

A young man about eighteen years of age, in skating, fell backwards on his head, and was stunned by the fall. He soon recovered and pursued his exercise. He complained of headache at night, and passed it in a restless state. He applied to a medical man the next morning, who gave him some strong purging medicine, and a mixture to produce moisture on his skin, and desired him to keep himself quiet. At night his head was more painful, he had sickness and giddiness. He was bled largely. The night was passed rather better than the former, but his sleep was unrefreshing, and his sickness continued,

and in the course of the day he was much worse and became delirious. His medical attendant being from home, I was desired to see him. I learnt these particulars, and on examining his head I found a small lump, soft, and apparently containing blood or some fluid. I desired to open the scalp, as he was delirious, the pupil open, and with a very quick pulse. This was not permitted; I, therefore, bled him largely, and ordered him more purgative medicine, and desired leeches might be applied to his head if he was not relieved; he had likewise some medicine to keep his skin moist. He had a dreadful night; all the symptoms were increased. I was permitted to open the scalp and examine the bone, but there was no depression nor fracture. I then stated it as my opinion, that, as the spot which was hurt could be perfectly ascertained, a trephine should be applied, but as the medical gentleman who attended him thought otherwise, no operation was permitted. I learnt that bleeding and purging was again had recourse to by order of a physician who was called in, that leeches were applied, and a blistering plaster placed on the head. The young man became convulsed, and all the symptoms increased, and as the physician thought that an operation was necessary, I was again called in, and I immediately removed a piece of bone. Under the bone there was a considerable quantity of blood, and, as I observed, there was more lying in a direction that I could not wash away, without removing more

bone, I reluctantly applied the trephine a second time, and found a very large clot. The young man seemed a little relieved. He was quiet for a short time, but the symptoms returned, and he died. The dura mater was not injured, nor did there appear to be, as far as I could observe, any blood underneath it.

A boy was pushed down in running across a street, and fell, the side of his head falling against a stone. He ran about at his play, and did not mention it to his friends, who were poor people. On the third day after the accident he complained of being ill, and of his head aching. I saw the boy, and found him sick, and feverish, and giddy. I knew nothing about the fall. As he was costive I ordered him some opening medicine, and gave him some antimonial powders, and should have given him an emetic powder, but his mother told me she had given him some chamomile tea when he was sick, which had vomited him freely.

I saw the boy the next day, he was sick and delirious, had passed a restless night, his pupils were dilated, and his pulse was very quick and weak. His mother accidentally mentioned the circumstance of his fall, and on examining his head I felt a small puffy tumour. I again purged the boy, and bled him, and as he was not relieved, I ordered leeches to be applied to his head.

He passed a very bad night, he lay muttering and starting at intervals. I opened the scalp and found the pericranium detached, and a slight de-

pression, which I could not suppose could have caused all the alarming symptoms that I had witnessed. I examined the head in every direction, but could not find any other injury. A vessel was divided, which was suffered to bleed, and a considerable quantity of blood flowed, which I thought relieved the boy, but in a few hours all the symptoms returned, he became restless, and convulsed.

I did not think myself justified in delaying the operation longer; but I was fearful that it would not do much for the boy, as I expected that there must be deep-seated mischief from concussion, the depression not being sufficient to account for the violence of the symptoms. On removing a piece of bone I found a considerable quantity of coagulated blood, which, added to the depressed skull, must have pressed considerably on the brain. The boy was a little better in a few hours after the operation; but he was not so much relieved as I hoped he would have been. His restlessness and delirium continued, but there was no return of convulsion. He was bled by leeches again, and his bowels were kept open. On the second day after the operation he became calm, his sleep was less disturbed, and in the evening he seemed to understand what was said to him. At intervals he became restless. His pulse, which had been low and frequent, became slower. On the third day he was materially better. He became calm and lucid, slept quietly, and re-

mained quite composed. From this time he gradually recovered.

I have selected these two cases from many others that I have recorded, as tending, in my opinion, to justify me in continuing the mode of practice which I have heretofore pursued. I have had under my own care, and have seen in the practice of others, and in the army and navy, in which I have served, and likewise in private practice, a great variety of fractures and other injuries to the head; and after the most serious consideration of what I have observed, and again referring to the works of the gentlemen to whom I have alluded, and reconsidering what they have written, I cannot help thinking that they have dangerously restricted the necessity of operating.

I have for some time been considering this subject, and have conversed on it with many practical surgeons; at the present time, I should not have ventured to have publicly animadverted on it, had I not found a very strong prejudice entertained against the doctrines which these gentlemen have promulgated. I did intend, sooner or later, to lay before the public what I have observed, or could collect, for or against these opinions; but as this work was about to be published, I could not let the opportunity pass of lifting up my humble voice against what appears to me to be most dangerous errors. At some future period this disquisition may be resumed,

and in the mean time I shall be very thankful to any gentleman who may meet with cases which prove any thing in favour of, or against these opinions, to transmit me an account of them, or if they are published, merely to refer me to the publication, as it is necessary that a matter of such vast importance to society should be minutely investigated.

It is a subject that cannot be determined by one or two men, however eminent, as the accidents which the head is subject to from external violence are so various, that it must be some time before general rules can be laid down. At present I must repeat, I have met with nothing which deserves the name of a dogma, or fixed principle, on which we can rely.

After this publication had been arranged, a case very much in point occurred to me.

A lady fell down stairs, and the back part of the right parietal bone came in contact with a step of the stairs. She complained of head-ache for two or three days, and had some sickness and giddiness, but as her husband was preparing for a journey on business, she did not express how much she felt, lest it might have induced him, very inconveniently, to have postponed his journey. This accident happened on a Thursday evening: Her husband proceeded on his journey in a day or two afterwards. She became worse; the pain in her head increased; she had violent lancinating pains darting through her head, and into the ball of the right eye. She

was incoherent at intervals, and had frightful dreams.

Thus she went on getting every day worse, till the Saturday week after she had fallen. On that day, she called with a servant upon me. I had never seen her before, and from her manner, which was strange, and from her reeling, as she was ushered into my room, I was at some loss to account for her appearance.

Her language was wild and confused. I learnt from the servant, that she had fallen, as related, about ten days before, and that her head was bruised. I examined her head and found a lump upon it nearly as large as a walnut. I urged her to return home, and visited her in a short time afterwards. The small lump was soft, and, as I have on similar occasions often found it, seemingly containing a fluid. I was not surprised in cutting into it, to find only a coagulated mass. I could not discover either fissure, or fracture, or depression, nor was the pericranium detached. A vessel was divided, which was suffered to bleed, and a good deal of blood was poured out. She was briskly purged. She was enjoined the strictest abstinence, and was kept quiet, and in the dark, for light affected her eyes extremely.

Her night after this was terrible. She could not be kept in bed; she was wild and delirious, and had nearly dashed herself down the stairs, having got out of her bed, and, by force only, was prevented from falling. Depletion was again

had recourse to, for I had, and have always had, strong in my recollection the facts and reasonings to which I have alluded; and I did concede in this case, as I have in others, my own opinion to a certain extent, in deference to those facts and reasonings.

Another alarming night was passed. I could no longer defer to the opinions of others. I immediately determined to proceed to remove a piece of bone, as it was evident, clear, and defined, where the injury was. The hollow of a trephine included it. On the bone being removed, the dura mater was found detached from it by a large and thick clot of coagulated blood. The lady was relieved. She had some darting pains in the right eye at intervals afterwards, but trifling in degree to what they had been, and, excepting when from mistake, her bowels were suffered to remain costive, she had not one unfavourable symptom afterwards. Her delirium vanished, her sleep was refreshing and composed, and her health was gradually restored.

My son being from home, I requested a medical gentleman whom I met with near this lady's residence, Mr. Dones, of this place, to assist me in the operation. He was kind enough to do so, as was likewise a young gentleman, Mr. Frankish, and they saw the effects of this operation.

A case resembling this has occurred lately in this neighbourhood, and as it is strongly illustrative of the matter on which I am writing, I have taken care to inform myself correctly of the facts.

As I understand the case is to be published, I shall only give the outlines of it.

A farmer fell against a gate and hurt his head. The next morning he had head-ache, and giddiness, and sickness. As he became worse, he sent for a neighbouring surgeon, who bled him, and gave him some opening medicine. This had no good effect. He became delirious, and a very distinguished surgeon was sent for from this place.

He opened the scalp, but there was no perceptible injury. The depletion was repeated, but the man became much worse. He was now much convulsed. This gentleman was again sent for. The trephine was applied, and on removing the bone, a large quantity of coagulated blood was removed. The man was somewhat relieved, but ultimately died.

A CASE OF VERY EXTENSIVE FRACTURE OF THE SCULL, WHERE A LARGE PORTION OF THE BRAIN CAME AWAY.

The following case is related, as shewing, that in the most deplorable cases we ought not to despair. It is related, as illustrating what has been the object of this paper, viz. to attempt to prove that there cannot as yet be general rules laid down to guide us in the infinitely varied accidents, which the scull is obnoxious to, and that

the principles, as they have been called, which have been attempted to be laid down by two very eminent men, are not sufficiently founded, and ought not to be absolutely relied on.

It is likewise of infinite importance in shewing that, with little external injury and without producing coma, and delirium, and convulsion, the most extensive injury may have been inflicted on the very brain itself, and from it much practical information may be gained. It tends to prove, that the opinions which I have alluded to, are rash and dangerous, for according to those opinions, this case should have been left to the efforts of nature. I believe that no person who reads this case, whatever sarcastic observations may be made on the others which I have related, will have the hardihood to assert, that this child, if left alone, and if depletion only had been had recourse to, would have lived. I believe, without presuming too much, that we may claim, *by our surgical work*, as it has been sarcastically called in some such cases, in a celebrated work, that we saved this child's life.

Miss P. the niece of a Roman Catholic Clergyman, about eleven years of age, was crossing the bottom of Bull-street, in this town, when a stage coach was passing rapidly down the street. The horses knocked her down, and one of the wheels passed obliquely over her shoulder and head, fracturing the clavicle, just touching the temporal bone, and driving the lower half of the parietal bone, which it broke in pieces, under the upper

portion of that bone nearly the fourth of an inch. This great violence was done to the bone without wounding the integuments.

The child was carried into a shop, and I was sent for. A crowd filled the shop, and I could not very accurately examine the child. I, however, ascertained that great injury had been done, as a ridge which was formed was evident. I desired the child to be carried to her uncle's, and immediately followed her.

As I was unknown to the whole family, I desired that the medical gentleman who usually attended might be sent for, and waited till that gentleman, Mr. Ledsam, came. As I had ascertained the extensive injury that the child had received, I had, before he came, shaved the child's head, and had sent for the necessary instruments, as I thought it was evident enough that an operation would be requisite.

Although this child had received such considerable injury from fracture and depression of the scull, and other injury, as was afterwards ascertained, she was not absolutely senseless. She was irritable and alarmed, but she perfectly recognised her friends.

I pointed out to Mr. Ledsam the ridge which was formed by the parietal bone, apparently projecting, by the lower portion having been driven under it. He hoped that so much injury had not been done as I stated, I suppose from the child's intellect not appearing to be much obscured. He consented, however, that I should

make an incision through the integuments, which I immediately made from the lower and anterior angle of the parietal bone, obliquely, in the direction of the fracture, to its termination in the lambdoidal suture. On the integuments being divided, a good deal of blood issued out, and mixed with it, a considerable portion of the brain. The integuments, although there was no external wound, were detached perfectly from the bone, which was broken completely from all connexion with the temporal bone, excepting at the anterior angle, where the temporal bone lies over it, forming a portion of the squamous suture, so that the fractured bone was almost loose, excepting where it was firmly driven nearly a quarter of an inch under the upper portion.

I thought I could get this lower part from under the upper, without removing or sawing out any portion of it, but I found that to be impracticable.

To remove this bone, wedged in as it was under the upper part, had I used a trephine, I must have applied it in two or three places on the unbroken bone. This would have been much to be regretted, as it was quite evident that the lower broken pieces of bone were so detached, that when liberated they must be removed, there being no chance of their reuniting with the contiguous and surrounding bone. There was one part of the fracture which formed almost a complete triangle; by sawing across the base of this triangular part I thought I could extricate the whole of

the broken bone from under the other. I easily cut this base through with one of Mr. Hey's saws, and with an elevator liberated the broken pieces.

A good deal of blood issued from under the fractured bone whilst I was sawing across the angle, and more of the brain came away. Our poor little patient fainted, and we had some fears that she would not outlive the operation. She happily did recover. The bone, as I have said, was so loose that it became necessary to remove it, leaving a space of the dura mater, of several inches square, denuded.

The integuments were brought together by three ligatures, which were indispensably necessary, and afterwards slips of adhesive plaster were applied. The child recovered her recollection soon after she was put to bed. She had a good deal of sleep, and in the morning she was infinitely better than we could have expected. Her pulse were very quick, but she was quiet and composed. Her bowels were kept open, and she had an antimonial medicine.

Extensive as this fracture was, and serious as was the injury to the brain, it is scarcely credible how mild all the symptoms were. I should scarcely have dared to state this case, had I not had a medical gentleman in attendance with me, as there positively is nothing to relate beyond what might have occurred from the most trivial fracture. When we removed the bandages, in a few days after the accident, adhesion of the scalp

had firmly taken place in most parts, and the whole had the most healthy appearance.

Some days after the operation I most unfortunately received an account of the death of my son, who was killed on board his Majesty's ship *Amelia*, in a sanguinary action with a French man of war. This distressing intelligence necessarily detained me at home some days.

On my visiting the child again, I learnt from Mr. Ledsam that nothing very particular had occurred, and I found her rapidly recovering, and in a few weeks she perfectly regained her former health, with her intellects acute and unimpaired. In this state she continues now, two years after the accident.

This was the first instance of recovery after any portion of the brain had been discharged, that I had ever seen, and so uniformly fatal had all the preceding cases been which I had had an opportunity of observing, that I had doubted the accuracy of some statements which I had heard of. It is not very easy to guess at the quantity of brain that came away in this instance, as some part was mixed with the blood that issued on the scalp being divided, and more escaped in removing the loose bone. It was curious enough that we could not ascertain from what part it came, as no part of the *dura mater* was wounded that we could perceive. From the considerable quantity that was separated, and from the injury done, I was fearful that we should have had a

fungus thrown up from the injured part, but there was no appearance of that serious disease.

From the state in which this child appeared after the accident, I apprehend that, according to the doctrine which I have alluded to, we ought not to have proceeded to cut into the scalp and to remove the loose and detached bone. The depression of the broken portion of bone was not so great as is mentioned in some of the cases which have been recorded by those gentlemen as having terminated favourably, without any operation, and the mental derangement was by no means so great as they have seen without alarm. We certainly could not have known that the brain had received such injury as we found it had in cutting through the integuments, and therefore, considering the symptoms, we certainly, according to their ideas, must have been rather officiously and unnecessarily making *surgical work of it* by all this operating.

Is it possible that depletion would have saved this child? Would it have been proper to have trusted to it in the first instance? Should we have waited to have seen what bleeding and purging would have done before we proceeded to examine the state of the bone?

After what has been written by these gentlemen, I cannot think that these questions are unfair. Supposing that we had been influenced by these opinions, and I have never seen a case of injury of the head without thinking and reflecting on

them, and that we had relied upon them, and therefore had trusted to bleeding, and purging, and abstinence only, and had observed all the delirium and convulsions that might have come on with firmness, believing, that in such a child as this the symptoms would gradually subside, and that by some means or other the bones would be replaced, or that the brain would adapt itself to the pressure. Supposing, I say, we had trusted to this, and that the child should have died (and that she would have died it is impossible to doubt) what would have been our feelings, if, after death we had examined the head of this child, and should have found it in the state in which it was, the bone broken in pieces, and a large portion of the brain between it and the integuments, and a large quantity of blood effused under the bone between it and the dura mater?

This is a severe case certainly, and such an one as does not often happen, but there was nothing that appeared before the scalp was opened to denote such extensive injury. From the manner in which the lower portion of the bone was driven under the upper part, there was a ridge perceptible on examining the head, but the bone was so bound down by this upper portion, that neither the broken state of it, nor its being completely detached from the integuments, could be perceived, as it appeared perfectly smooth and convex. The child, as I have said, appeared alarmed and confused, but she recognised her friends, and I have many times seen infinitely

more mental derangement from very trifling accidents.

I do believe, from all the appearances which this case presented, that no person would have proceeded to the operation who had been influenced by the opinions I have alluded to ; and I think no surgeon will doubt what would have been the consequence if no operation had been performed.

A medical gentleman has favoured me with the following case while this work was printing. It is so awfully illustrative of the danger of the doctrines I have so frequently alluded to, and tends so completely to prove that absolute dependence ought not to be placed on them, that I have inserted it. In this case such a decisive plan was adopted, that it is impossible to say that the practice was not carried on long enough and with sufficient firmness.

A female child, ten years of age, fell and hurt her head. The scalp was bruised over the parietal bone. A surgeon was sent for, who found her comatose. The integuments were not wounded, and no fracture nor depression could be perceived. She was purged briskly. The next morning she remained in a state of stupor. Another surgeon was called in, and she was again purged and bled ; this was repeated the same day, and as the stupor continued, a branch of the temporal artery was opened and blood taken from thence.

On the third day she was again bled twice, and

she had purging medicine. She was a little relieved, but as she relapsed into the same state again, she was bled on the fourth day. On the evening of that day convulsions came on, and she died.

The head was examined after death. On removing the scalp a simple fracture was discovered, but without the smallest depression. The bone being removed, under it, and between it and the dura mater, they found about an ounce and a half of blood.

The gentleman who was called in on the second day took with him two of his surgical friends, and all of them were gentlemen of the first respectability and intelligence. I know not if they were deterred from opening the scalp and removing the bone from the opinions which I have so repeatedly mentioned; but as they are gentlemen of extensive reading, I should presume that they had those opinions, and the cases on which they were founded, strong in their recollections.

This case speaks volumes. I relate it precisely as I have received it; the inferences to be drawn from it are palpable enough, without any observations of mine.

On some Appearances in Hernia which are not often met with.

ALTHOUGH Mr. Astley Cooper has paid so much attention to Hernia, and has, in his magnificent work on that subject, so highly to his own reputation, and so usefully to the world, almost noticed every possible modification of that complaint, still there will occasionally arise anomalous cases that ought to be recorded.

The following cases have occurred to me, and may, possibly, be of some use to the practical surgeon.

About twelve months since I received a letter from Mr. Wilcox, surgeon, of Knowle, in this county, requesting me to meet him about ten miles from hence, to see a female patient of his who was suffering from strangulated femoral hernia.

I rode to the place, and found a woman about sixty years of age, with every appearance of strangulated intestine. The bowel had been down, I was told, three days. She was very sick, and had vomited up every thing which she had taken. She had not had any stools, and her belly was tense and sore to the touch.

On examining the part I found the swelling had precisely the appearance of strangulated hernia, but on touching it there was not that hard elastic

feel which strangulated intestine produces, and from several circumstances, I was pretty certain that it could not be omentum that formed the tumour. I accurately examined the swelling repeatedly, and became convinced that it was impossible for the symptoms that were present to arise from that tumour. I thought I perceived a small tumour deep down by the side of the swelling which I had examined. I pressed firmly upon it, but it did not recede. I again examined it, and I still found it firm and irreducible.

The larger tumour, which was palpable to the sight and touch, remained precisely in the same state when the pressure was made upon it. It had nothing in its feel like intestinal or omental hernia, although it precisely resembled the former in its appearance. As there was every symptom of strangulated bowel present, as the woman was in exquisite pain, and in the greatest danger, and as I was of opinion, from all these circumstances, although there was much obscurity, that strangulated intestine must be the cause of all these symptoms, and as I was disposed to think that the small deep-seated tumour might be this cause, I was induced to propose to Mr. Wilcox to cut down to the larger tumour, to discover what it was, and to proceed afterwards as circumstances might indicate.

Mr. Wilcox was perfectly aware that the tumour did not, at the time I saw it, resemble strangulated hernia when touched, but he assured me that it had been much more tense and absolutely

hard. He, and a gentleman who had attended with him, had endeavoured, by pressure, to return the intestine, which they supposed the contents to be; and I have not the smallest doubt that they were right, as I believe that the tumour which presented itself to view was the sac of a hernia, the contents of which they had returned.

As Mr. Wilcox agreed with me in the necessity of proceeding to cut down to this tumour, I proceeded to dissect carefully through the integuments and fascia. The tumour rose up as I extricated it from the surrounding parts precisely as hernia does, and I was even then almost convinced that there must be intestine in it. By opening the parts I had, however, obtained a perfect knowledge of the small deep-seated tumour. I was quite certain that it was a hernial sac, containing intestine, strictly bound down. I carefully divided the ligament without opening this sac, and returned it. The woman immediately felt relief. I became, therefore, freed from much embarrassment. I was convinced that the material cause was removed, and I had leisure to examine the larger tumour. Its configuration was precise and defined; it extended from its fundus regularly towards the ligament, and I have no doubt, if I had pursued the dissection of it, I should have traced it under that ligament, as it was, I believe, an hernial sac, which had contained some of the abdominal contents, and which had been returned by the efforts of Mr. Wilcox and the gentleman who assisted him.

It certainly could not have been any peculiar arrangement of cellular membrane, for its form was too regular and defined, and I am sure it was not a gland. I have performed the operation, and have dissected the parts so frequently, that it was absolutely impossible to make that mistake. It had nothing glandular in its appearance, nor in the feel of it, and resembled nothing in its feel but an empty thick membrane.

I had determined to open this sac, but on conferring with Mr. Wilcox, as our patient was quite easy, we did not think ourselves justified, for mere curiosity, to do this, as it was evident that no strangulated intestine or omentum was contained in it.

I was quite sure that there was not any thing from the abdomen in the tumour, because after I had liberated the strangulated intestine by dividing the ligament, I introduced my finger under it, for the especial purpose of examining the parts to ascertain that fact.

The wound was therefore dressed, and the person was put to bed. She became, as I have stated, quite easy, the sickness left her, and in the course of a few hours afterwards she had stools. Mr. Wilcox was assured that the upper portion of the bowels was evacuated, as she had eaten currants in gruel the morning before, and some of them were observed in the fæces.

Mr. Wilcox had some difficulty in persuading this person to take some opening medicine, and

in consequence of it she became costive, and her bowels were painful with feverishness, but after she had had copious evacuations all these alarming symptoms disappeared, and she soon recovered.

I have examined Mr. Cooper's splendid and excellent work on hernia, but I cannot find any thing like this case recorded in it; that is, if I am right in my conjecture, that in this case there had been a double hernia, and that both the sacs had contained intestine or omentum at the same time. I cannot doubt, from what Mr. Wilcox told me, that there had been intestine in the larger tumour, which had been replaced by the efforts which he and the other gentleman had made; I am quite sure that the small tumour contained intestine. There is nothing in the nature of hernia to render this impossible. If there had been an old hernial sac, with its inlet not quite closed, it is possible that the same force which had driven a portion of intestine into it, might, at the same time, have propelled another portion down, and formed a new hernia.

In the first part of Mr. Cooper's work, in plate 5, fig 7, there is an engraving of two sacs very near to each other, one of which is said to have contained intestine, and the other had become contracted from wearing a truss, and therefore was no longer capable of receiving intestine. If it had so happened that the recent hernia had been formed before the mouth of the other had

been contracted, this would have formed a case somewhat similar to this which I have been describing.

This shews the necessity there is, in obscure cases, accurately to examine the parts, as the strangulated intestine was so small, and was so deep-seated, that it was with difficulty it was perceived.

I met with a case somewhat resembling this in performing the operation for strangulated femoral hernia in this town. There was a fulness near the sac of the strangulated hernia that I was operating upon, and which seemed to dip under the ligament. I thought at the time that it was an old sac, but as it did not cause any embarrassment, nor interfere with the operation in which I was engaged, I of course did not think it necessary, nor indeed proper, to trace it. The person was a patient of Mr. Taylor's, who, with Mr. Aston, both of Birmingham, was present.

This person's sister had the misfortune to have strangulated femoral hernia a few years after, on whom I performed the operation, and she fortunately recovered as well as her sister. I merely remark this as a singular coincidence. There was no peculiarity in the form of the pelvis in these two sisters, which would have induced a surgeon to consider them as more liable to femoral hernia than other women. There was no particular projection of the symphysis pubis, nor of the crista of the ileum, nor were the bones peculiarly wide.

The following case was rendered obscure from there having been a considerable collection of fluid in the tunica vaginalis, and at the same time there was a portion of intestine strangulated at the upper part of the ring. The parts were so very tender from inflammation, that it was difficult to ascertain what the disease was.

I was requested by Mr. Taylor, surgeon, of this town, to visit a patient of his, who was supposed to be labouring under strangulated inguinal hernia; but I was informed by him that it was a very complicated case, and that there were great doubts about the nature of it. I found the man in exquisite pain, his belly tense, vomiting unceasingly, and I was told he had not had stools for some time.

A large tumour occupied the whole scrotum up to the abdominal ring, which was much dilated. The swelling could be traced up above the ring towards the opening formed by the tendons of the internal oblique and transverse muscles, and which some anatomists have called the internal ring. On examining this tumour as accurately as I could in the tender state in which it was, I was convinced that it was formed by a fluid, and I supposed that it was contained in the tunica vaginalis. This fluid seemed to have insinuated itself under the vaginal sheath covering the seminal vessels, distending it and the abdominal ring, and thus making a continuous column, which was so gorged and filled with fluid as to give it the appearance and feel of in-

guinal hernia. This appearance was so accurate, for it was not contracted at the ring as in hydrocele, giving it a pyriform shape, and all the symptoms of a strangulated hernia being present, certainly did give this case very much the appearance of a strangulated intestine.

On examining to the top of this column, I thought I perceived a break in the continuity of the parts, and a difference likewise in the feel of them. It occurred to me that possibly there might be a portion of intestine strangulated in this upper ring, as I knew that frequently it was the case, for I had several times, in operating for inguinal hernia, found it necessary to divide it before the bowel could be liberated. I re-examined the part, and I became more convinced that this was the case. It was quite evident that this collection of fluid could not produce all the symptoms which I witnessed, such as sickness, and tension of the abdomen, and costiveness, but if there was, as I apprehended there was, a piece of the intestine by some means or other strangulated, then the symptoms were easily to be accounted for, the case became clear, and my way was obvious and distinct.

From the man himself very little information was to be obtained, and Mr. Taylor having only seen him a short time before, knew little more about it than I did. I, however, thought that I had judged right of the case, and if I was right, I could not easily err in the operation. I first attempted by pressure on the upper part of the

tumour to return the intestine, if intestine it was. This had no effect. I tried the tobacco glyster in vain. Pressure was tried again, till it became evident that nothing could be done by it.

As the symptoms were very urgent, I determined to proceed to the operation, for we could not learn how long the bowel had been strangulated, if it were strangulated. This we knew, that he had been without evacuations for some days, and, therefore, it was possible, that this obstruction was caused by the disease we were about to try to relieve, be this disease what it may.

I made an incision carefully into the lower part of the tumour, and on dividing the tunica vaginalis, a prodigious quantity of puslike fluid, rushed out. The swelling high up above the ring remained unaltered. This confirmed me in the opinion which I had formed, and I felt quite certain of what I had to do. I introduced my finger to the top, where the swelling still remained, and discovered a portion of intestine strangulated. I carried a curved knife on my finger up to the part, and very easily dilated the stricture, and the intestine was returned into the abdomen.

The poor man was relieved by the operation, and by great care and attention on the part of Mr. Taylor, he rapidly recovered. I have seen him a few days since, and I find his health has been tolerably good. He has twice had a collection of fluid, formed, as Mr. Taylor informs

me, in the old sac, but there has not been any return of the strangulated hernia.

Mr. Aston, surgeon, of this place, was kind enough to assist at this operation.

As I have related two peculiar cases of strangulated hernia that have occurred in my practice, it will not, perhaps, be uninteresting if I relate a case, which I have lately seen, where a very large portion of omentum was so much diseased as to make it necessary to remove it. It is so far interesting as it proves what has been asserted by that great surgeon, Mr. Pott, and by others since his time, that very large quantities of omentum may be removed without hemorrhagy.

I must beg to be understood that I should certainly tie any vessels which might be found where it is necessary to remove any part of the omentum, as I believe fatal hemorrhagy has taken place from omitting it. It is, however, a fact, that sometimes the vessels are so minute, that none can be found on the most accurate examination.

A very respectable man, for his occupation in life, had been long tormented with a very large irreducible omental hernia. From his situation, which was that of night constable, he was very liable to colds, and in this winter, during a violent fit of coughing, a portion of intestine was forced down and became strangulated.

Mr. Geast and Mr. Owen, surgeons of this place, were called in. The man was bled and some purging medicine was given him, which it

was thought might possibly act, from the circumstance of its being an old omental hernia, and by the action of the purgative, the peristaltic motion of the intestine might be increased, and thus the bowel might be liberated. It, however, had not that effect, and I was requested to see him.

I attempted to reduce the intestine, but in vain. The tobacco glyster was used, and the other usual remedies were had recourse to without success. I then explained to him the necessity there was for the operation to be performed, as he had been in this state several days, and urged him to consent to it. Unfortunately, from a misapprehension of something that had been said to him, joined to that natural desire that every man must feel, in such an unhappy situation, of postponing the evil hour as long as there is a chance of assistance being afforded by any other means, he positively refused to submit to it.

A whole day and night were, therefore, passed in pain and restlessness, and in unavailing attempts to return the intestine.

On the succeeding day he consented, and I performed the operation. There was a prodigious quantity of omentum down, and in the centre of it was a fold of intestine, very little discoloured considering the length of time that it had been strangulated. The omentum was of a very dark red colour, and some part of it was nearly in a state of mortification. It adhered to the sac in many places. The omentum was in so large a

quantity, and in such a state of disease, that there was no question of what ought to be done.

I removed the whole of the diseased part, which weighed one pound and a quarter; and although this great mass was removed, and, consequently considerable vessels might be expected to have been divided, it was with difficulty that any could be found to be tied. As I had before removed considerable quantities of omentum in old hernia, and knew how difficult it sometimes is to find the vessels, I very deliberately and slowly cut through these, that the vessels might be seen and secured as I proceeded; but with all my care, and with the quick attention of the gentlemen about me to assist me, we could only tie two very small ones.

As two vessels only could be discovered I did not feel any alarm, and in fact there was no necessity for it, as no hemorrhagy did take place. I believe that I once saw a person die from internal hemorrhagy, where a considerable portion of omentum was cut off, and the part returned without any of the vessels being secured.

It has been asserted by great authority to be useless tying these vessels. I believe it is now a determined point of practice, that all vessels in these cases should be secured if possible. In the present case it was impossible, as the vessels were so small as to elude all possibility of applying the tenaculum.

There was a peculiarity in this case which I had never met with before in old omental hernia.

After having divided the abdominal ring, I found such a degree of stricture at the upper part of it, I mean at the superior or internal ring, that I was obliged to dilate it before I could return the intestine.

My patient being put to bed was soon much relieved by having copious evacuations from the bowels. He had never had a full hard pulse from the time I first visited him, and indeed I have often observed in strangulated hernia, when it has continued for any length of time, that the pulse is generally quick and feeble.

This person was a very large, corpulent man, of bad habit. He was, as I have said, a night constable, which, from his constant exposure to the night air, the want of refreshing sleep, and the almost constant custom of drinking spirits which such employment generally produces, had altogether made him unfit to undergo a great operation, and superadded to these, he was affected with a very violent and unceasing cough, which had indeed forced down the intestine. I became therefore very anxious about him, even after his bowels had been evacuated.

On visiting him about six hours after the operation I found him cold, with a low trembling pulse, and it was evident the shock of the operation had almost overwhelmed the powers of life; I therefore gave him some wine, and as he had had many stools, and his belly, which had been tense and painful, was quite soft, I ordered him an opiate.

At seven the next morning I found that he had had more stools, and had slept comfortably, but his pulse were extremely low, his features sunk, with a glassy eye. These symptoms we know are terrible. I ordered him some warm spirits and water, which was occasionally repeated in the day with the most decisively good effect, and the opiate was repeated. Indeed, the opiate was obliged to be given very frequently, as his cough continued violent and unceasing, and it must be evident that nothing could be more unfavourable, and give more annoyance after any great operation, and more particularly after the operation for strangulated hernia. A person had strict orders to watch him constantly, and to make a gentle but firm pressure on the part, as I momentarily expected the intestine to be forced down again.

The third morning I found him much better. He had passed a comfortable night, and his pulse were become more favourable. He took nourishment, and every symptom, excepting the cough, was as favourable as could be wished. All tension and pain were gone from the belly, and he had natural alvine evacuations without the aid of medicine.

On the fourth day he was going on well. The wound was dressed, and we found adhesion had taken place in some parts, and the whole looked healthy. He thus went on, and there was every reason to expect a favourable termination of the disease, if the cough could be removed; he took

nourishment, slept comfortably, his bowels were perfectly regular, and his mind was composed, and indeed cheerful.

Thus he proceeded, the wound going on well, and no appearance of disease from the hernia, but nothing would allay his cough; it continued in defiance of all the remedies which were prescribed for him, and on visiting him on the ninth day I found his pulse sunk and scarcely perceptible, his features fallen, and delirium coming on. He continued in this state all the night, and died on the tenth day.

From the sudden change which had taken place, I thought that effusion had taken place into the cavity of the chest or pericardium, as the change was so very rapid. I was anxious to inspect the body, for several reasons, as this poor man had been shot through the chest some years before by robbers who were attempting to break into some premises in the district which was under his protection, and I understood that his lungs had been wounded by the ball, and that he had never been without cough or some pulmonary affection from that time.

I could not attend the examination of the body, but from the gentlemen who were present, I learnt that there was not any effusion of fluid into the chest or pericardium. The bowels were in a natural state, and there was little appearance of disease any where but about the lungs. I examined the lungs myself, and the cells were lined

with tough mucus. They were inflamed, and they had adhered to the ribs. There was no trace of any wound having been made in the left lobe, through which the ball was supposed to have past.

I suppose if the cough could have been relieved that this poor man would have lived.

*An Account of the Treatment and Removal of
a very large Tumour of the Scrotum and Tes-
ticle, and adjacent Parts.*

THE case which I am about to relate was enveloped in such obscurity, that the cause and nature of it were not ascertained without considerable difficulty, and as the treatment of it may possibly afford some useful hints, I have inserted it in this work.

I was called to a farmer about six miles from hence, whom I found stretched on his back, with a prodigious tumour of the scrotum and of the thigh, and extending from above the abdominal ring half way down the thigh, in length fifteen inches by eight or nine in width.

The information I received was, that he had been tormented with a very large tumour of the scrotum for twenty years preceding, and that in assisting to load a waggon with turnips, and standing on the wheel for that purpose, his foot slipped, and in falling this great tumour came in contact with the projecting nave of the wheel with great force, and thus the inflammation and swelling was caused which I found him labouring under.

This had happened two days before I saw him, and the parts were in the highest possible state of

inflammation. In such a state it was impossible that he could bear me to examine it with accuracy, and I was therefore precluded from gaining any information in that way as to the cause of this great tumour, and from himself and his friends I could gain no satisfactory explanation. He thought that the swelling had commenced from above, but his wife assured me that she was certain it had first begun to swell from the lower part. From such conflicting evidence nothing satisfactory could be learnt.

From the appearance of it I thought it must be an immense omental hernia, and as he had taken some salts which had acted powerfully before I saw him, I was relieved from the fear of there being any intestine strangulated; and it was pretty clear that there was no strangulated omentum, if omentum it was that caused this swelling, as I was quite sure that the present inflammation was brought on from the fall against the wheel.

This simplified the case as far as related to the immediate treatment. It was evident that the first thing to be done was, to allay the high inflammatory action, if possible. Bleeding of course was had recourse to, a low diet was enjoined, and above all I impressed on the minds of the person and his friends the necessity of reducing the temperature of the part by the unremitting application of cold water, as it was impossible that life could be sustained if mortification came on in such an immense mass as this was.

By these means the inflammation abated, and

in a few days it was fortunately reduced to its usual size; but even after the swelling, which the recent injury had produced, had subsided, the tumour was of great dimensions.

This person had suffered so much from this complaint that it had made his life miserable. Upon the least exertion more than usual, he had great pain in it, and most alarming inflammation had frequently arisen from colds or slight accidents, which his laborious and active life rendered him very liable to. He requested my opinion as to the possibility of removing it; being perfectly convinced that he could not possibly exist much longer if that could not be done, and being determined to submit to an operation if I thought it advisable.

I most accurately examined the tumour in every direction. I could not feel the abdominal ring, but I was assured it was not an omental hernia, as the swelling continued of an uniform size far above that opening. No testicle could be felt, nor was there any pain in any part of it when it was supported, nor at any other time, but when brought on from cold or injury. The tumour was firmer than omentum, and the form more regular, and there was not that doughy feel, if I may be allowed the word, which omentum has. There did not seem to be a fluid, as no fluctuation could be felt on repeated examinations, and it had not the elastic feel which fluid, contained in a membranous sac, produces. About the lower part of it I thought I could feel something

like a fluid in the centre of it, but so deep and so obscurely was it felt, that it could not be perfectly ascertained to be so.

I desired him to call upon me when he came to town, hoping that it would be a little more reduced in size, and in the interval I considered of the case. On re-examining him when he called upon me, I became more confirmed in my opinion that there was some fluid in it. He told me that he thought the swelling began from above, but so many years had elapsed that he had no distinct recollection of it. He had, he said, been laid up with it from bruises and colds he knew not how oft, great inflammation had always come on, and he found a considerable permanent increase of the size after each of those confinements.

As I became more assured that there was some fluid in it every time I examined it, I thought that the disease might possibly have been originally an hydrocele. That the fluid had not been rapidly effused into the tunica vaginalis, and that from want of care, and the scrotum having been repeatedly injured by bruises and other accidents for he was a most active and industrious man, that from such repeated inflammations, coagulable lymph might have been poured out into the cells of the dartos, which had consolidated and thickened the parts so much, and might have formed such massive walls, as to prevent, by their thickness, the farther distention by the effusion of water. That from the same cause often repeated, which he stated to me to have been the

case, the inflammation might have extended to the testicle; and that the parietes, by the accretion of fresh matter, had gone on increasing and thickening till this immense mass was formed, presenting an irresistible barrier to the farther accumulation of fluid in the vaginal sac.

If my premises were right, I thought I could not err in my conclusion. Although I thought I had formed a correct opinion of the nature of the case, I wished him to take the opinions of other surgeons, as he had suffered so much and so often, and so convinced was he that it would soon kill him, that he had firmly made up his mind to have it removed if I would venture to remove it. I was almost a stranger to him, and, indeed, was very little known in this town, as it was soon after I had fixed my residence here; and an unfortunate operation, however necessary to be done, and however well it may be gone through, affords such a scope for malevolent misrepresentation, that I thought it imprudent to attempt to remove this immense tumour without the approbation of others.

I begged him to shew it to Mr. Mynors, a surgeon of great practical knowledge and of long established reputation, and before he called I put Mr Mynors in possession of all I knew of the case, and the opinion which I had formed of its cause and nature. I told Mr. Mynors that I had made up my mind to remove it, if I procured his sanction, and I described the plan of operation that I had determined on, if he consented to it,

Being now quite assured that there was some fluid, I proposed as a preliminary step to draw this off with a trocar or by incision. This would reduce it in proportion to the contained fluid, and I hoped that by thus exciting action in its centre a considerable discharge would be produced; and, likewise, by the stimulus of the incision, that the absorbents might be excited to action, and perhaps considerable absorption of parts might take place.

To all this Mr. Mynors, with great candour, assented. I proposed to leave the case after the fluid had been evacuated, for some time, to avail ourselves of what the absorbents would do for us in removing any part of it, and to proceed to the ulterior operation, that was, to dissect it out, sooner or later, according to the urgency of circumstances. Mr. Mynors acquiesced in this. When, however, my patient called upon him, and he saw the immense tumour, he paused and doubted, as, indeed, well he might. No surgeon but would have hesitated on seeing, for the first time, such a mass as this.

My patient, however, was firm and decided, and nothing of hesitation shook his resolution; he knew, if it could not be removed, that he had not many months to live.

We agreed that the first part of the operation should be carried into execution in a few days, as soon as our patient was prepared. I introduced a long trocar, made for the purpose, to the very hilt, before I perceived the point to be

in a cavity. About four ounces of puriform fluid were discharged. This reduced it but very little. Great inflammation came on after this, which, to a certain extent, we wished, as I hoped that in consequence of it such a discharge would come on, that a considerable portion of the mass would be melted down, and thus materially lessen the quantity to be dissected away. A prodigious discharge did come on in a few days, and reduced our patient so much, that we thought we must have proceeded to the ulterior part of the operation without delay.

This intimation of proceeding to the latter part of the operation, merely from the debility which the great discharge had produced, may appear to some readers, if it had been carried into effect, to denote bad reasoning and worse practice. But I believe surgeons who have had much to do in the removal of large masses from the bodies of men reduced and made irritable by long continued disease, by anxiety and pain, know, that sometimes when increased action has been excited in them, if the general strength decreases, and there comes on shiverings alternated with heat, with a pulse increased in quickness and decreased in firmness, that sloughing sometimes rapidly comes on, and when once that process has begun it is difficult to set limits to it.

Perhaps this rapidity of falling into decomposition may arise from a less perfect organization than the natural parts of the body possess, and, therefore, that the vitality of them is weaker.

But whether this be correct reasoning or not, practical surgeons know, that under these circumstances, if the sloughing process has not commenced, and if the integuments be free from disease, such tumours may be removed, with the expectation of safety, that adhesive inflammation will go on, and the source of irritation being removed, health and strength will be restored.

For this reason I was afraid that I must have had recourse to the removal of the tumour long before it had been reduced so much in size as I hoped and expected, and under circumstances which certainly would not have been chosen, if it had not been imperiously rendered necessary.

The bark and wine were given liberally, and fortunately by these remedies, and by removing him into the country, the disagreeable and threatening symptoms were arrested, the discharge which had been thin and ichorous became favourable, there were no longer any sloughy membranes intermixed with it, his pulse became slower and stronger, and all the symptoms of debility and irritation ceased. He soon regained as good a state of health as he usually possessed. There continued a regular drain from the opening, and the absorbents seemed to have acted powerfully, and in about seven or eight weeks this once formidable looking tumour became so reduced in size, that it could not have deterred any surgeon, however timid, from dissecting it out.

As the object was attained which we had in view, and indeed the plan which I had suggested

had answered my most sanguine expectation, and as the tumour seemed to have been reduced as much as it was possible to expect, I proceeded to dissect it out.

In doing this nothing particular occurred. It was merely dissecting out a prodigious large testicle with a mass of matter adhering to it, which was condensed and identified with the surrounding parts and glued by successive inflammations to the thigh, and swallowing up, as it were, the body of the penis, not any part of which could be seen, excepting a very small portion of the glans penis. Very fortunately the penis, though adherent and enveloped as it was, continued perfectly sound ; and I succeeded in separating it without injury, and it was ultimately saved. Adhesion took place in a large portion of the divided parts in a few days, and he rapidly recovered, and is now a healthy and happy man.

I was unfortunately deprived of the able assistance of Mr. Mynors, as he was seized with a fit of the gout, which confined him to his bed many weeks, and to which disease he at length unfortunately fell a victim. I however was fortunate enough in having two intelligent young men to assist to me, Mr. Dadley, now a surgeon in Manchester, and Mr. Mole, surgeon at Weymouth.

A curious Case of Retroversion of the Uterus, when that Organ was not in an impregnated State.

IT is scarcely necessary to record extraordinary cases without they lead to some practical result, and therefore I shall not trespass on my readers frequently with such narrations.

The following, however, I hope I may be excused relating, as I have never seen nor heard of any thing like it, excepting a slight description of a case somewhat resembling it, I think in some of the works of Mr. John or Mr. Charles Bell. I took a note of it at the time I read it, but I have mislaid the memorandum.

I received a message from Doctor Smith, of this town, requesting me to call upon a Dispensary patient of his, to introduce the catheter into the bladder, the woman not being able to evacuate the urine.

I attempted to introduce the instrument, but although the meatus urinarius was almost projecting, as it were, so much so that its opening could be felt almost like the mouth of a cartilaginous tube, the instrument would not proceed into the bladder. I tried to introduce a probe with no better success.

In retroversion of the uterus I had before observed this curious protrusion of the opening of

the urethra, and I enquired if the woman was pregnant, suspecting that the uterus was retroverted; but she gave me perfectly satisfactory reasons to prove that she was not with child. I then explained to the woman the urgent necessity there was for an ocular examination of the parts, which she consented to; but I could not then introduce an instrument of any kind more than a quarter of an inch into the urethra, and there was nothing particular to be observed, excepting the protruded mouth of the urinary passage.

I then attempted to introduce my finger into the vagina, but it was stopped by a firm solid body, which blocked up the whole passage, and which I was quite convinced was the uterus in a state of retroversion.

I desired that Doctor Smith might be sent for. When he came, I stated the case to him, and he satisfied himself of the fact by examination. I then proceeded, in his presence, to return the uterus into its proper situation. This was not done without considerable difficulty, as the body of it was much increased in size, and in forcing it through the brim of the pelvis I observed precisely that sensation, which I know not how to describe, that I have before felt in replacing the gravid uterus when it has fallen into the same situation.

The feel I allude to is, as if a quantity of loose irregular pieces of some solid matter were mixed with a doughy substance, and the whole crowded into a thick fleshy pouch or bag, and crammed through a narrow aperture. This does not ex-

actly express the precise sensation, and I am ignorant of any simile that will convey the idea. It must, I think, be familiar to practical men.

On the uterus being returned, the catheter passed with perfect ease. To my great astonishment not more than a teacupful of urine was evacuated. The belly was examined. The outline of the bladder could be traced with the greatest accuracy, as this poor creature was much emaciated from long continued disease. It was precisely pyriform, not at all resembling the shape of the human bladder when distended, but similar to the bladder of the calf when inflated. From its apparent magnitude, it would have appeared to have contained about three pints of water.

Doctor Smith suggested to me to introduce a male catheter, as he thought it was possible that the urine might be contained in some sac or cavity that the female catheter would not reach.

This was done, but no water came away. I passed the male catheter up to the very fundus of the bladder, for although there was no fluid in it, it was distended, as we felt it to be through the abdominal parietes. The sensation which was produced by the catheter touching the fundus and sides of the bladder was as curious as any thing else in this remarkable case. It produced the sensation of pushing the instrument into a carnosous cavity, with the sides thick and soft, as for instance, as if it had been pushed into one of the ventricles of the heart.

We found, on examination, the liver enlarged,

and the lower edge of it very hard, which could be traced with perfect accuracy. Below the liver, close to it, but not connected with it, we felt an irregular rounded substance, which I suppose was an enlarged ovarium.

The woman was yellow from jaundice, was much emaciated, her stools were white, and the small quantity of urine which was drawn off was of a dark coffee colour, mixed with blood. With all this disease she had little pain of any kind.

Doctor Smith prescribed for her, and I saw her several times afterwards. After some days she passed a tolerable quantity of urine, which continued to be very dark coloured, and occasionally it was mixed with blood. I examined the abdomen several times afterwards at the interval of several weeks, and I found that the bladder and liver continued precisely in the same state.

I learnt from Doctor Smith that, contrary to all expectation, she became a good deal better after several months, and that there was some prospect of her being restored to tolerable health, and in that state he informed me she was when he resigned his situation as one of the physicians to that institution.

After his resignation, the care of this poor woman devolved on Mr. Baynham, the visiting apothecary of that charity. That gentleman has been kind enough to favour me with the following particulars.

Mr. Baynham states, that when she came under

his care, "he observed much irregular enlargement of the liver. That during the last eighteen months of her life she had been jaundiced. That there was a communication from the liver to her lungs, as she *expectorated* bile in large quantity, with a tough mucus. A large and painful tumour was seated in the left hypogastic region." Mr. Baynham goes on to state, "that he had no reason to suspect enlargement of the uterus when making pressure on the abdomen. She had not any uterine discharge, nor at any time did she complain of any derangement of that organ."

It was quite evident, from what Mr. Baynham so obligingly informed me, that she could not live long, and as it was very desirable to examine the body after death, I requested some persons in the neighbourhood where she lived, to let me know when that event happened. Mr. Baynham likewise took care to make enquiries; as she had ceased to be a patient belonging to the Dispensary. Unfortunately, neither of us were informed of her death till she had been buried some days.

Two Cases of Amputations of the Thigh which were attended with some extraordinary Appearances.

I Have to intreat my reader's pardon for inserting two short cases which were attended with particular appearances, although, I fear, no very useful practical inferences can be drawn from them. I will not again trespass on their indulgence.

Although perhaps no absolute practical inferences can, in the present state of our knowledge, be drawn from these cases, it may be not entirely without some use to attract the attention of medical men to the possible cause of the disease, which rendered it imperiously necessary to amputate these limbs.

As far as my knowledge extends, I do not recollect ever to have heard or read of it having been suspected that the important coincidence in these two cases, viz. the deficient supply of blood, in consequence of the main trunk of the chief vessel of the limb being too small to convey a sufficient quantity for the nourishment of the parts, having been the cause of the gradual and irreparable decay of the one, and of the absolute death of the other.

I do not state this to be absolutely the exciting cause of these diseases. I merely, at present,

state the remarkable and important coincidence between these cases, as far as relates to the extreme smallness of the main artery in both.

It would lead me far from the path which I prescribed to myself when I sketched the plan of this work, were I to pursue the speculations which these cases have given rise to in my mind, and which, from some other analogous facts that have come to my knowledge, I do not deem absolutely visionary and absurd. As this is merely a practical work, I shall suppress those opinions, and theories till more facts have either established or refuted them.

In the year 1798, I was at Athy, in the county of Kildare, in Ireland, with a regiment of dragoons, to which I was surgeon. The assistant-surgeon, Mr. Spencer, was intimate with Dr. Johnson, a physician of that town. This gentleman practised surgery, which is, I believe, usual in the small provincial towns in Ireland.

Mr. Spencer rode into the country with Doctor J. to see a man who had been wounded by slugs a few days before, and on their return the Doctor was requested to visit a poor man in a neighbouring cabin, who was lying there with a bad leg. It was a dreadful case, and the Doctor directed his friends to convey him to Athy, and humanely promised to take care of him. Mr. Spencer gave me a terrible description of this poor fellow's leg, and informed me that, as amputation was necessary, Doctor J. intended to request me to perform the operation.

I met the Doctor the next day. I never shall forget the scene. In a dark hole, with no more light than could be admitted through an aperture in the wall of six inches square, on some straw on the bare earth, there was extended the most squalid, wretched figure, that ever met my sight.

Near his wretched straw was a fire formed of Kilkenny coal, which ignites without flame. The bluish livid light which was thrown from this fire on the spectre before me, enabled me to discover the skeleton of a leg thrust out of the straw, naked, denuded of all vessels, and muscles, and skin, as are the bones collected in a charnel house.

I enquired from what this dreadful disease arose. The poor creature puckering up his rigid and sinewy muscles into a horrible sardonic grin, told me, "*and plase your honour I am bewitched.*"

Had I lived some years back, it certainly would not have required a very great share of superstitious credulity to have thought that the man had guessed at the right cause of his complaint, for certainly such effects did scarcely appear possible to be produced by natural causes,

I doubt not that this scene is well recollected by Doctor Johnson. I certainly shall never forget it, as I suppose will be easily credited, when I declare, that this description is neither coloured nor heightened to produce effect, but is related precisely as it struck me at the time.

A light being brought, I examined this misera-

ble being. The bones of his leg were, as I have stated, bare. The whole of the muscles, and ligaments, and membranes were fallen off from the extremity of the toes to the protuberance of the tibia, excepting something which connected the bones with each other. At the upper part of the tibia there was some skin and membranes in almost a putrid state. At the articulation of the knee ineffective inflammation and ulceration were going on, being an effort of nature, as it has been called, to throw off this inanimate mass.

The muscles of the thigh were extenuated to the utmost that they could be consistent with life, as indeed was the whole body; but above the knee there was no ulceration.

This poor man was ordered some porter, and wine, and nourishing food, for to have amputated the limb in his then weak state, would have been to have doomed him to certain death.

As soon as he could bear the operation I amputated the limb above the knee. After it was taken off, in proceeding to tie the vessels I looked for a long time before I could find a single vessel. I slit up the sheath of the ^{cc}trunks, and discovered a small vessel, so unimportant, that I believed I should not have tied it if I could have found any other. On loosening the tourniquet I could not find any thing else to secure, and therefore I, of necessity, applied the dressings, and he was placed on his bed.

It is useless to detail the other circumstances of the case, as indeed there is nothing particular to

relate. Adhesion¹ went on as well as in the healthiest man, and in a short time he went home well.

I learnt from the man that this leg had always been cold, and subject to swell. That he knew nothing of the cause of the present disease ; that it swelled and then became inflamed, and then became as if it were dead ; that the soft parts gradually wasted away. So firmly was he and all his friends convinced that the disease arose from witchcraft, that he had never applied for any medical assistance before they requested Doctor Johnson, who was accidentally riding by, to see him.

I examined the bones afterwards. Some of the toes and metatarsal bones were gone. There was very little appearance of disease on any of the other bones. I know not how they were retained in contact with each other. The whole had the appearance as though the cartilages had not partaken of the disease of the muscles and integuments, but as if they had been shrivelled up and dried. From what I could collect, which was very little, I should suppose that from some cause or other, when disorganization did begin that it went on with great rapidity, and that death and sloughing soon took place.

How this poor man was supported through this process I know not, or how life could be sustained under it, cannot be guessed at. He assured me that little bleeding took place at any

time. This was the sum of all that I could collect from the man or his relations, who were the most ignorant poor creatures that I had ever met with.

SECOND CASE.

I was requested by Mr. Geast, surgeon, of this town, to see a female with him who had a bad leg. I visited her, and immediately recognized the person, who was a most interesting delicate woman, whom I perfectly well recollected having seen about a year and a half before, by the desire of another medical gentleman who then attended her.

I saw her only twice at that time, as it was evident that amputation was necessary, which I recommended, and as she would not consent to it, I took my leave. I was surprised to find the disease not much increased in all that time, and I was infinitely more surprised to learn, that since I had seen her she had been delivered of a fine healthy child. Her health, however, was worse than when I had seen her before; she was thin, as, indeed, was to be expected, for her's had been a life of continual uninterrupted suffering.

The leg, from the knee to the extremity of the toes, was swollen and covered with ulceration.

The tibia was bare in several places; and sinuses burrowed in every direction, pouring out a most fœtid discharge. There were hollows about the ligaments of the foot, and sinuses in all directions along the tarsal and metatarsal bones. There was not one sound spot from the knee to the toes, all was a mass of incurable disease.

How this most delicate woman had borne all this discharge, and suffering, and misery, I know not. I assured her that the greatest good I could do for her, would be to immediately remove this source of wretchedness. She assented, and it was removed without delay.

After having amputated the leg, I looked out for the vessels. I looked in vain. Not one vessel, not even the femoral artery was to be seen. I thought of the case which I have related. I saw a small tendinous looking substance, and as there was a small aperture like a point in it, I drew it out with a tenaculum, and my son tied it. I again carefully examined the stump, but could not find a single vessel. I unscrewed the tourniquet a little, and no blood issued. I loosened it, and I found that I had tied the femoral artery! The aperture of this vessel might have been stopped with a pin. Small as this vessel was, and reduced as this person had been, it surprised me to observe the powerful pulsation of it. I shall not state any thing of the after treatment. She fortunately recovered.

These two cases I think are of considerable

interest. This poor woman, when she was a girl, had always had remarkably cold feet, which were frequently swollen. Her father, I understood, had always had his legs swollen and cold, although otherwise in good health. The disease of her leg began early in her life, and nothing had ever retarded it.

*On the Duties of Surgeons in certain Cases where
Persons have been bitten by mad Animals.*

IT must often happen that surgeons are called to persons who have been bitten by mad animals, in parts of the body which it is not possible to remove with safety to the life of the person. When it happens that such wounds are inflicted on the extremities, and it is ascertained that the animal is mad, if those wounds should penetrate between the bones, so that it becomes impossible to ascertain whether the parts can be effectually removed, or if the parts cannot be removed without injury to vessels necessary to life, it becomes a matter of most serious consideration, whether, under such circumstances, the animal that had inflicted the wound, being proved to be mad, and the wound penetrating, for instance, between the bones of the tibia and fibula, where it is impossible to ascertain that the whole bitten part can be removed; in such cases it is doubtful how far the surgeon ought to proceed. Is he justified in amputating the limb, if he cannot remove the whole of the injured part?

The following statement is published, as it may relieve the mind of a surgeon who may reside in a situation where it is not possible to collect the opinions of different professional men on the subject, and to whom such an event should occur.

A case of this kind did actually occur, and the opinions of four physicians of the very highest character for science and learning, and of several surgeons, some of them of most distinguished eminence, were collected.

It was almost the unanimous opinion of these gentlemen, that the surgeon is not authorised in such cases to amputate the limb, but that he should dissect the parts out as far as it is possible to be done with safety, and if it be likewise consistent with safety, afterwards to apply caustic to the parts.

This opinion may be considered something like a settled rule, to direct the opinions of individuals, as it was proposed to all these gentlemen separately, and there was scarcely a difference in the opinion of any of them.

Eight years since, many persons were bitten in this town and neighbourhood, by dogs which were supposed to be mad. In the course of nine months I was applied to by thirteen persons who had been bitten; fortunately, in parts where I was enabled to removed the injured portion without danger, and all of them remain well.

Among this number was a little girl, between four and five years of age, who was worried by a large dog that was said to be mad. The upper part of the child's leg was torn, but the principal injury was inflicted between the bones of the leg, above the outer ancle, where the canine teeth of the dog had penetrated very deep.

From the direction and depth of the wound, it

became very doubtful whether the parts could be removed with safety; indeed, it was doubtful whether they could be removed at all, with such accuracy as to be certain that every atom which had been touched by the tooth of the animal could be dissected away; on this, the entire removal of the part, it is well known that the safety of the person depends. The evidence as to the madness of the animal was clear and decisive.

These considerations made me very doubtful of what ought to be done. It was rather a moral than a surgical question; and as I am no casuist, I determined to consult several other medical men.

I sent to request my neighbour, Mr. Blount, surgeon, to examine the child. He agreed with me as to the uncertainty there was of dissecting out the parts effectually, as the wound was ragged, deep, and narrow.

Under these very embarrassing circumstances, we determined to proceed no farther at that time, as we felt quite certain that the operation might be postponed for a hour or two without danger, and in that interval we agreed to consult several of the most respectable physicians and surgeons.

After having collected the opinions as I have stated, we proceeded to the operation, and I dissected out the parts as accurately as I possibly could do it, after which the kali purum was applied to the parts, as we hoped, from its rapidly diliquescing, that it would insinuate itself into

any scratches or vacuities which might have escaped the knife. Fortunately, this was done without any injury.

I dare not take upon me to say, that we removed every atom that was touched, but great pains were taken to do so. The child, however, recovered and continues well.

In the removal of parts which have been bitten by mad animals, I have always made it a rule to insert a probe as far as I can push it into the wound if it be narrow, then, by dissecting under and round the probe, and bringing out the part upon it, it is ascertained, beyond the possibility of doubt, that every atom is removed.

This can be done in muscular parts; in other parts, among tendons, and nerves, and arteries, the surgeon must be guided by circumstances. One thing I must mention, that I have, in two cases, removed the parts which had been bitten, a considerable time after the injury had been received, and with success. In one case, the dissection was not made till the second day, and in the other three days had elapsed; happily, both these cases did well. I must distinctly state, that I by no means wish to have it understood that I think it proper to delay the operation. I think excision cannot be too soon resorted to.

Fortunately for mankind, I believe that the dreadful complaint Rabies* Canina does not hap-

* The word Rabies implies madness in general, but I believe it is restricted by medical men to designate that species of madness in

pen so frequently as our fears induce us to believe. I apprehend that there are many acute diseases of animals which produce derangement of their perceptive faculties besides this fatal disease; and, as in this, there is no diagnostic or distinguishing symptom to point it out from all those which render animals delirious and infuriate from other causes, I believe that this appellation has been given almost as a generic name to all those diseases, however dissimilar in other respects, that agree in having delirium and wildness in common.

Were not this the case, from all that we know of this most frightful disease, it would be impossible that such numbers should escape with impunity as are bitten by mad animals, and who only rely on what we know to be useless and inefficient nostrums.

If the greater number of animals that become mad are affected with the true disease, Rabies, human beings must be infinitely less susceptible of receiving the disease than is supposed. In the course of twenty years I have known and heard of many persons who have been bitten by what have been supposed to be rabid animals, who have not had the parts cut away, but have, in many instances, depended solely on the absurd and superstitious custom of being dipped in the sea three times, for three successive mornings,

animals which has hydrophobia as its distinguishing symptom, and which may excite, by the insertion of saliva into a wound, or by producing some diseased action, the same disease in human beings.

and others who have relied on remedies equally ridiculous.

I believe that the safety of those persons must have depended on the circumstance of the animal by which they had been bitten not having been affected by rabies ; as I am afraid, that when a person has been bitten by an animal ill with that disease, that nothing but excision of every atom which has been touched by the teeth, will secure him from the dreadful disease, Hydrophobia.

On some of the Injuries which have been produced from the Custom of swallowing the Stones of Fruit, as of Plums, &c.

A Very strange custom obtains among the lower orders of people in this town and neighbourhood, and extends, indeed, in some instances, among the middle classes, of swallowing shot, and large quantities of cherry-stones, and in one case that I have met with, the stones of the Orleans plum, *to keep down the lights*, as they call it; for there is a very common disease among a certain set of people hereabouts, as all medical men very well know, called *rising of the lights*.

The disease which is designated by this term is, generally, dyspepsia; but as they are quite latitudinarians in their terms, and are not very strict in their nosological definitions, this fanciful title is given as a generic appellation to all sorts of diseases for which no other name can be found.

I have very good reasons for believing, that many persons whom I have attended have died from this pernicious custom; but as a very strong prejudice prevails against permitting the examination of dead bodies, I have never obtained permission to ascertain the fact. In one case, I have had the most decisive proof, that death was the consequence of this strange practice.

A most respectable and worthy man, a large manufacturer, sent for me, he was much emaciated, and had frequent vomitings, and, at times, the most obstinate costiveness. Every thing which he swallowed disagreed with him. A few spoonfuls of milk, or gruel, or other simple food, would sometimes produce violent pain; indeed he was never entirely free from pain in the stomach and bowels, and was constantly teized with sour eructations. Tasteless watery discharges were thrown up in great quantities. His days were passed in sickness and pain, and his nights in restless watchfulness. The whole *Materia Medica* had been ransacked in vain to find a remedy for his numerous complaints, or as palliatives to give some ease. In this state I found him, tired of his physicians, of his surgeons, and of life.

He had long had more or less of dyspeptic complaints; and, in listening to the long narrative of his sufferings, he happened to mention, among other things that he had taken, that in the preceding autumn he had eaten many Orleans plums, and that he had swallowed all the stones, as he had been recommended to do so by some good neighbour, who had assured him that his disease was the rising of the lights, and that those stones being swallowed, would be more efficacious than any other remedy, even than leaden shots, in removing his complaints.

He could not tell me any thing of what had be-

come of those stones. He did not know whether they had been passed by stool or not. I hoped that it was not possible that they could be retained in his body. At all events, it did not seem to be possible that they could be retained in his stomach, as he had taken several emetics before I had seen him, and about the stomach and liver seemed the chief seat of the disease. I ventured to give him one more emetic. I gave him a large dose of ipecacuanha, and desired him to drink after it a diluted solution of sulphate of zinc. This acted powerfully, and unloaded his stomach of much viscid mucus and slimy matter.

He became better, and I had almost forgotten the story of the plum-stones, when I was called in great haste, and he told me he had found something, as he thought, burst within him, great sickness came on, and he vomited nine large plum-stones. In a few days afterwards, five more were thrown up.

These stones had been swallowed nine months before, and had not been acted on by the fluids of the stomach, or by the fluids of any other parts where they had been retained, as their edges were as perfectly sharp and their angles as defined as when they were swallowed.

This unfortunate man was relieved for a short time, but he sunk, emaciated and exhausted.

Where these stones could have been inbedded so many months, it is not easy to conjecture.

The body was not opened, I believe, as I did not attend him till he died, he having heard of some person whom he hoped would be more capable of relieving his sufferings than I was.

COMPLICATED CASE OF FISTULA IN ANO, FROM
PLUM-STONES HAVING BEEN SWALLOWED.

I was requested by Mr. Taylor, a medical gentleman of this town, to see a patient of his who had a fistula. On examination, I found it one of the worst cases that I had almost ever met with. There were several ill-looking fistulous hollows round the verge of the anus, and one running up very high by the side of the rectum. On introducing a probe high up this sinus, I perceived that it came in contact with a hard body. I re-examined with great care, and it occurred to me that a piece of the os coccygis must have been separated by disease or accident, and from its stimulus, had excited inflammation and the whole disease that I observed. I thought that the feel of it against the probe, produced the sensation of its being a harder body than bone. No symptom of disease had ever been observed in or about the sacrum, and the person was quite certain that he had never had any accident which could have broken it off.

The person was young and healthy, and it was

evident that this body, be it what it may, was the cause of these fistulous hollows.

I determined to remove it if possible, but as it was very high up, it was not quite certain that I could reach it.

The shallow hollows were opened, but I could not reach the hard substance. Mr. Russell, now a surgeon in Birmingham, whom I had met accidentally as I was going to the operation, and who accompanied me, suggested to me to use the concealed fistula knife, which was invented, I believe, by Savigny, and although I have no great opinion of complex instruments, this certainly enabled me to make the incision deeper and with more facility than I could have done with the common curved probe-pointed bistoury.

Having by this means opened the sinus completely, I easily reached the substance with a pair of forceps, and extracted several considerable fragments of stony matter, but from its having lost its shape and figure, it was not possible to ascertain from its appearance, nor to conjecture what it was. From some experiments which I afterwards made upon it, and on the stones of different fruits, I have not the least doubt of it being fragments of stones which had been swallowed, and which had made their way into the loose cellular membrane about the rectum.

This man had never had any calculi pass from the bladder, nor had he ever been troubled with

nephritick complaints, nor did the analysis afford any result at all similar to what any kind of calculus would have given.

Three years after this, I found a complete cherry stone in a fistula of the anus, but this was not more than an inch from the surface. In both cases, after the removal of the irritating cause, the men rapidly recovered.

A Case of supposed Injury to a Ramification of one of the cutaneous Nerves of the Arm.

THE following case having been involved in much obscurity, having occupied much time, and at last very alarming symptoms having arisen, I have thought that the publication of it may possibly be of some service; as the plan which was adopted, and fortunately with success, I am of opinion would, under similar circumstances, be attended with the same result.

Mr. F. Marrian, of Buck-street, in this town, whilst pruning a gooseberry-tree, wounded his thumb with a thorn, on the outside of it near the nail. It inflamed, and some poultice was applied to it by direction of a surgeon who attended him. After some time the small piece of thorn was extracted, but the inflammation continued a considerable time, and at last the parts healed.

He, however, still felt a good deal of pain in the part, and it began to extend to the first joint, afterwards to the second, and at last darting pains extended to the wrist and up the inside of the forearm. Various remedies were tried in vain to arrest the progress of the disease.

He thus went on for nearly a whole year, the pain continuing, and the disease constantly extending. About the beginning of the second year

a small tumour was perceived under the skin, a little above the inside of the wrist. In a few weeks, a second tumour was observed a little higher up, and in a short time afterwards three others large, and several small ones, made their appearance on the inside of the forearm, the highest being near the tendon of the biceps.

The pains were now very great, extending up the inside of the whole arm, and darting into the shoulder, and into the muscles of the breast.

He now consulted another gentleman, who, after trying several remedies without any effect, recommended that the thumb should be amputated. This was objected to, and again other applications were made to the parts, and amongst the rest mercurial frictions on the arm were recommended. This was persisted in for some time. Two tumours about this time appeared above the bend of the arm.

He had thus gone on gradually getting worse for more than two years, the pains being now very severe, darting up to the shoulder and chest, shooting in radii into the pectoral muscles, and he thought into the cavity of the thorax. All the tumours were more or less painful; all were increasing rapidly, and those above the bend of the arm were become very large, and adhered to the subjacent muscles.

He was in this situation when I was desired to see him. There was some little soreness about the thumb, which seemed to have been chiefly kept up by the nail having grown into it. This had

been removed, and Mr. Marrian carefully kept it from irritating the part by cutting it away when necessary.

As all the tumours were in a state of inflammation when I first saw him, my attention was directed to allay the increased action in the parts, and that being done, the mercurial ointment was directed to be resumed, as I thought, with the gentlemen who had attended him before, that it was some disease of the absorbents.

As this had no beneficial effect, and as the appearances and symptoms were very alarming, I determined to remove the uppermost tumours by excision or by caustic ; as the more I pondered on the case, and thought on the original injury, and the whole train of symptoms that followed it, I became the more convinced, that the disease which I had to combat had arisen from injury to a nerve ; and thus, if my opinion was correct, the plan to be followed was evident. The method of treating the painful disease, *Tie douloureux*, which had been recommended by that excellent anatomist and profound physiologist, Doctor Haighton, was in my recollection, and although this was a very different disease, there was some analogy between them ; and I felt assured, that if my view of the complaint was correct, a favourable termination would be the result of a somewhat similar practice.

I was induced to suppose that the disease arose from an injured nerve, first, because it was

evident that it could not have arisen from a wounded tendon, as the thorn did not enter near the flexor or extensor tendons; and the whole train of symptoms had no resemblance to those which arise when tendons are punctured. If the vaginal sheath of a tendon has been pierced, on inflammation coming on, the pain is excessive. The inflammation extends rapidly downwards, involving the whole thumb or finger, and extending down to the hand, if it be not stopped by cold applications, or if an incision be not made deep down through the theca, to evacuate any fluid which may be effused or formed there.

Secondly, it did not appear to be a disease of the absorbents, for these reasons. I could not find that Mr. M. had ever observed any hard or sore cord-like vessel running up the arm, nor had he perceived any redness in the course of these vessels. Neither of these appearances, however slight they might have been, could have eluded his observation; but if it had been possible that either of these appearances could have occurred without his having noticed them, he must inevitably have known if any of the absorbent glands had ever been enlarged.

There had not, however, been any swelling in those glands which are in the course of the artery, nor in the axilla. The great length of time which had elapsed from the infliction of the injury, likewise proved that the absorbents could not be the seat of the disease; as had it been situate in that

system of vessels, it must have been terminated, in some way or other, long before the period that it had occupied.

As these reasons seemed quite decisive against the possibility of the tendons or absorbents being the seat of the disease, I concluded that it must have arisen from injury to a ramification of one of the superficial nerves. As the tumours above and below the bend of the arm were precisely in the direction of the internal cutaneous nerve, and as they were immediately between the skin and fascia, I could not doubt that that nerve was the seat of the disease.

There certainly was an objection to this conclusion, as that nerve generally terminates about the wrist; but it is known, that the extreme branches of nerves sometimes vary in their terminations, and it was possible, that in this case, a filament of it might have extended up the thumb.

As this nerve arises from the great axillary plexus, its connexion with the circumflex and with the thoracic nerves, completely explained the darting lancinating pains which were felt round the shoulder and about the chest, and the anatomy of the axillary plexus tended therefore the more to confirm my opinion.

If my reasoning were correct, the mode of practice which was proper to be adopted was evident. Analogy, and precedents, and reasoning, all distinctly pointed out the propriety of cutting off all connexion with the origin of the nerves; and

whether this was to be done by the knife or by the caustic, was left to me to determine, for, from my patient being a man equally distinguished by firmness and intelligence, I had no wavering nor obstruction to fear from him.

In fact, his suffering so long and so much from the deep lancinating pains about the shoulder and chest, superadded to what he felt in the whole arm, had excited such alarming apprehensions in his mind as to the probable result of the disease going on without controul, that he was perfectly passive; and he was willing to submit to any thing that I proposed to him, if there were any probability of it putting a stop to the disease.

Such was his bravery, that he even proposed to me to cut out all the tumours, if I thought it necessary.

To cut out the lower tumours, or to interfere with them, or to amputate the thumb, would have been wrong, according to my idea of the disease; it would have been contrary to, and would have been impugning the reasoning that seemed to me conclusive. It would have indeed been wretched practice if my reasoning were right, as if that were right, it was quite evident, by cutting off all communication of the disease with the parts above, that the symptoms below would cease, and that the tumours would soon be removed by the absorbents.

I hesitated whether I should dissect the two upper tumours out, or rather a large double tu-

mour, for it was not quite divided, or whether I should destroy it by caustic.

If I had merely divided the trunk of the nerve, which my theory would have induced me to do, independent of some experience which I had gained on the subject before, I might thus have expected immediate cessation of all the alarming symptoms in the chest and shoulder joint; but in a case somewhat similar to this, I had not succeeded by that practice. After having divided this nerve, the pain above ceased, but a new pain arose about the back of the arm, perhaps from some branches of this nerve intermixing with others of the external cutaneous nerve or perforans Casserii, or from that nerve having partaken of the disease.

In that case, after having failed in my attempt to cure by merely dividing the nerve, I dissected out the part that was painful, and even then I did not succeed till I had destroyed a considerable surface by caustic.

For these reasons, and also as I should have had a very extensive dissection to make, I chose to rely on the caustic in the first instance, determined carefully to watch its effects, and if I found the pains in the shoulder and chest to increase, which I feared would be the case, then to immediately divide the body of the nerve above the tumour with the knife.

I chose to use the lunar caustic in preference to the other caustic, as all the others are so diffusible; the nitrated silver being quite manageable;

and I could dissect with it, as it were, any part which I chose to remove. As soon as the integuments were penetrated, a thick, almost ligamentous, substance presented itself very different from fascia. This was gradually destroyed. It spread very considerably, and was more than half an inch thick. Layer after layer was detached till the muscles became denuded. Before this time all the pain had ceased above the part. Of course I had nothing to do with the muscle. The diseased portion was removed, and with it the disease disappeared.

I cannot refrain from stating how much of the success was owing to the firmness of my patient. Not content with the slow progress of the sloughing, he forwarded it by every means in his power, and sometimes by no gentle means. We have too frequently to lament that our most matured and best plans are thwarted by the weakness of those we are attempting to serve; but I had in this case nothing to plead in excuse of failure, on that account, if I had failed; had that been the case it must have been fairly attributed to my want of knowledge.

Most fortunately I did succeed. The pains entirely ceased. The uneasiness in the tumours on the forearm gradually subsided, and, as I expected, they have all disappeared, absorption having almost removed every vestige of them; and now, February, 1815, fifteen months have elapsed, and Mr. M. remains completely well, as I have examined his arm to ascertain the state of it.

Were such a case to occur again, I should perhaps treat it differently. I should, in the first instance, I think, divide the nerve above; and if that did not prove sufficient, I should apply the caustic on the incised part, so as to destroy a considerable surface. The parts, however, were so extensively diseased when I first saw Mr. M. and morbid action had been so long continued, above two years, that I believe, under similar circumstances, I should adopt a similar mode of treatment. Perhaps dissecting the upper tumour out, and applying caustic on the exposed surface, might have hastened the termination of the business; but I am not quite sure of this.

I hope the relation of this troublesome case may give some useful hints to surgeons as to the treatment of such complaints in future. It gave me much trouble and uneasiness, as at one time, the termination could not be contemplated without the most serious apprehensions.

From the history of this case may not some useful hints be taken, if not inferences drawn, as to the treatment of tetanic affections, if arising from injuries about the extremities?

Is it not probable that locked jaw would have come on in this case, had not the morbid matter, or the diseased action been impeded by some unknown cause in its passage to the sensorium?

Was not the inflammatory action, which took place in regular progression at the first joint of the thumb, then at the second joint, then at the

wrist, &c. &c. &c. all the parts above remaining quiescent, that cause?

If unfortunately any case of locked jaw should occur in my practice, which originates from mechanical injury, I shall not fail to recollect this case of Mr. Marrian's; and if medicine do not soon relieve, I will, if I am permitted, search out the course of the nerves, if possible, with the knife or caustic; and if the injured nerves be so deep-seated as to make that impossible, I will excite active inflammation as near them as I can with safety, and thus at least attempt to stop this hitherto generally fatal disease.

Some Observations on the Mercurial Disease.

I Am not acquainted with any book which has been published of late years, that is more practically useful than Mr. Mathias's, on the Mercurial Disease.

He has completely proved that this is a distinct, substantive disease; that it is no symptom of another complaint, as it has been unfortunately long continued to be supposed; that it depends solely on a peculiar exciting cause; and that, if the disease has not been permitted to extend its ravages too far, by removing the cause, the effect will cease.

He has defined this complaint with great accuracy, and has demonstrated that, under circumstances which he distinctly points out, the disease will certainly be produced; thus shewing surgeons that this malady, which has been long and fatally known, and most unfortunately confounded with another, is no modification of syphilis, but is a disease *sui generis*, and which will certainly be increased and aggravated by the improper continuance of the poison that generates it.

He has, with admirable precision, laid down the distinction between venereal and mercurial ulcer, which, if attended to, will free the physi-

cian and surgeon from much doubt and anxiety, and will be of infinite service to the world.

This disease had certainly been known for many years, and the proper treatment of it, that is, so far as instantly abstaining from the use of mercury, had by some medical men been accurately ascertained, but no clear and distinct marks having been pointed out to distinguish it from some irregular syphilitic symptoms, the best informed and most acute practitioners were often puzzled in ascertaining the land-marks, and boundaries that separated them.

Many medical men knew nothing about it. With them all complaints that followed original lues, and many that had never even been preceded by any thing like that disease, were massed and confused as symptoms of syphilis. Mercury, when properly given, being certainly the only cure for that disease, was prescribed in every case, however inapplicable, and every man must have often heard the hundred times repeated phrase, that *the habit had not been sufficiently saturated with mercury*.

I can call to mind a vast number of cases where I have seen what is properly called phagedenic ulceration, of the most formidable kind, rapidly extending in all directions along the penis, destroying the glans, and amidst the corpora cavernosa, and extending up the muscles of the abdomen, and dissecting them as it were, descending to the scrotum, and extending its ravages in all directions, whilst the palid, weak,

and trembling patient was still swallowing or absorbing the cause of all his suffering.

Mr. Hunter was well acquainted with the mischief that the improper use of mercury produced, and in his writings and lectures pointed out the many cases where it was pernicious, and I have not the least doubt, that a man of his sagacity and truly philosophic mind, well knew the distinction between those symptoms which arose from irregular syphilis, and those caused by the mercurial poison. In some of his lectures he pointed out so accurately the mercurial disease, that it seems extraordinary that he did not describe it as a distinct malady.

If, however, this great man (of whom I never think but with sentiments of profound veneration) did not ascertain this to be a specific disease, he has done infinite service by warning surgeons of the danger which the improper use of mercury excites, and I believe that most men of the present day have obtained their knowledge on this subject either from his writings or from his opinions, and which have been more or less amplified by succeeding writers. It is evident that Mr. Mathias, accurate as he has been in his observations, has derived much valuable matter from the opinions of that illustrious man.

Diffused as the writings of Mr. Hunter have been, and extensively as his opinions have been spread, it is surprising how many medical men there are who, at this time, are utterly ignorant of the distinction between venereal and mercurial

ulceration. I know that, in the opinion of many medical men, even now, after Mr. Mathias has published his excellent work, all the bad symptoms which follow lues proceed solely from that disease, and that the only cause of those symptoms making their appearance arises solely from the constitution not having been sufficiently saturated with mercury. The observations of Mr. Mathias, I hope, will in future settle the minds of men on the subject.

There certainly is a circumstance worthy of remark, which has not, as I know of, been discussed, but which is well worthy of Mr. Mathias's observation.

In the other various diseases, in which it is necessary to give mercury, we seldom observe this, the mercurial disease. I do not recollect ever to have seen the true mercurial ulcer formed in any other disease, where mercury has been necessarily long given, and even when given in the saline preparations of it, excepting in scrofula. In that disease I have frequently seen it; but in those affections of the liver, where it is frequently given for a considerable length of time, I have never seen it, either in the shape of ulceration or of general affection.

Is there some greatly increased irritability induced? or does it depend merely on the previous debility that is brought on? In scrofula I have seen the mercurial ulceration clearly and decisively brought on, and yet that disease is distinctly marked by inirritability.

This work does not permit me to indulge in speculative remark, but it appears to me that this subject should be pursued and investigated by some person who has more leisure and more ability than I have, as the non-appearance of the disease, in cases where mercury is long and largely given, may give rise to much scepticism; and although I think it is absolutely proved that it is a distinct complaint, there ought to be some explanation given on this subject.

Although Mr. Mathias has done so much for the profession by giving us clear ideas on the subject, and pointing out how we are to avoid it, I do fear that there is yet a great deal to be done, before we shall know how to cure it when it is produced. I have most unfortunately seen a good deal of this disease, as indeed most men must who live in large towns; and as long as the saline preparations of mercury are continued to be given, and while people will resort to ignorant and uneducated men, this must continue to be the case, for the basis of all the nostrums which are advertised is muriate of quicksilver, disguised in some way or other. Indeed I know that some regular practitioners generally use this dangerously active medicine.

When this disease is completely formed, I know no complaint more difficult to be removed; and from having seen many remedies quite useless that are spoken favourably of by our greatest writers, and which are constantly prescribed by the most scientific and eminent men of the age, physicians

and surgeons, I am become so sceptical, as almost to doubt whether any remedy which has yet been discovered does absolutely cure this complaint.

It has appeared to me, that in those who happily recover, the disease has been worn out by constitutional energy, the exciting cause being omitted, for I lament to say, that I have never seen in one case where the person has been seriously reduced, that he has ever outlived the disease, if he could not be supported by the usual tonics, and by light nourishing diet and pure air.

I have seen such an uniform failure of the several remedies which have been recommended, that I have always availed myself, wherever I may have been at the time, of the opinions of the most distinguished men of the neighbourhood, and have frequently had the cases stated to the most eminent men in the metropolis of the country in which I have been, but I am sorry to say I have too frequently seen all our efforts fruitless.

I rejoice, therefore, to find that Mr. Mathias had suggested a new practice, viz. frequent bleeding. His book contains so much internal evidence of truth, and accuracy of observation, that I perfectly rely on what he writes, and shall not fail to resort to this remedy. Unfortunately, it seldom happens that a second person is called in to these cases till the disease has produced such excessive weakness, as to preclude the possibility of safely having recourse to it. I have

not seen a single case for some time, in which I could have recommended it. In one most distressing case that I have seen lately, the gentleman had been bled several times before I saw him, not for this particular complaint, but for an affection of the lungs, which he had occasionally been subject to, more or less, all his life,

In that case, it had no beneficial effect on the mercurial disease. This, however, I by no means state to deteriorate the practice, as I believe that nothing could have saved that unfortunate gentleman; and, at all events, a single case cannot prove any thing.

Of another remedy that Mr. Mathias joins with all writers and all practitioners almost in extolling, I must speak with more decision. I mean the Sarsaparilla. I have given that drug and seen it given in every form, and in the largest quantities, and for very long periods, in innumerable cases, and I do declare that I never could discover that it had any effect when given in decoction, which an equal quantity of any warm mucilaginous fluid would not have had, and when in powder I never have been able to ascertain that it possessed the tonic powers that it is supposed to possess.

In writing this I do so with almost fear and trembling, as I know that I am opposing the opinions, or rather, if I may so call it, the acquiescence with the established opinion (for I know some great men who continue to order it, that

think as meanly of it, I believe, as I do, but continue to give it, because they know not what else to give) of some of the first men of the age.

I hope I shall not be suspected of writing this from any such weak and miserable motive as the desire of being considered more acute and observant than others, by affecting singularity. No man can possibly despise, more than I do, all such illegitimate claims to distinction. I have paused and considered well before I have thus written, and although I shall, perhaps, be abused as a rash and inconsiderate man, or condemned as a stupid and ignorant one, I cannot retract; I must persist in saying, that as far as my observation and experience enables me to judge (and I have carefully watched in an infinite variety of cases) that I have never, in one single instance, been able to trace out one good effect from its use when given alone.

If I have erred in drawing this conclusion, I have not erred wilfully, for I have many years watched the exhibition of this drug. Nor have I arrogantly despised great authorities. I have doubted the accuracy of my observations; I have thought that I have drawn false conclusions; I have paid all this deference to the opinions of others; and, with all my doubts, I have still continued to use this medicine. But after conceding, and thus giving up what I have thought right, to the opinion of others, I cannot allow facts, as they appear to me, that contradict those autho-

rities, to escape me. I think I have seen much valuable time lost in trusting to this medicine.

Of the effects of cicuta, I am most happy in being able to concur with Mr. Mathias. I think it is of infinite importance in every stage of this disease. It certainly most admirably allays the increased irritability which is so evident. It has most important advantages over opium. It does not increase heat, as opium almost always does. It does not produce such costiveness, which appears to me to be a great evil, and, as far as I have observed, ought to be carefully avoided.

Cicuta, I think, joined with the lighter tonics, or with the bark when it can be given, is of the greatest service. The secretions from the skin and the bladder, Mr. Mathias particularly directs our attention to, and, from all I have seen, I think his observations on that point deserving of great attention, most particularly as relates to the moisture of the skin, for nothing will, I believe, be of service, if the skin be dry and hot.

Mr. Mathias does not notice the effects of iron or arsenic. I have seen preparations of iron do much good, if they can be given so as not to excite heat, and when the skin can be kept moist.

Of arsenic, I have not had sufficient experience to speak decisively. In one case that I witnessed its effect, I thought it was injurious.

Of the local effects of mercury, that is, of its local application in some cases of chancre, I have not seen such injurious effects as Mr.

Mathias mentions. When chancre occurs in very irritable habits, and the surrounding parts run rapidly into inflammation, the mildest applications are necessary, with fomentations of poppy heads. There is a kind of chancre that often occurs, where I have found the application of calomel and lime water of much service, and likewise the occasional use of the red nitrate of quicksilver. When the chancre, without much surrounding inflammation, continues excavating the part, forming a yellowish covering, with a thin discharge; in such a state I have often found the red nitrate, finely levigated and applied in powder, of great service. The yellow covering is thrown off, and, by repeating it, the wound becomes a simple sore, and rapidly fills up. I have tried many other applications of various kinds, and in that state I know none so effective.

Again, when there are several chancres under the prepuce pouring out a great discharge, producing tumefaction and pain, I have seen the application of calomel and lime water of great service; and hundreds of times I have seen much benefit result from injecting this mixture under the prepuce, where phymosis has been brought on by the secretion from chancre lodging there, when mild washes have had no effect, and whilst mercury has been rubbed in, or been taken internally, without producing any amendment.

This effect of the local application of the nitrate of quicksilver and of the black wash, as it is technically called in the London hospitals, must be

so familiar to surgeons who are in the habit of seeing many venereal cases, that I cannot think it necessary to insist on it. I have seen its beneficial effects in so many hundred cases, without producing any thing like mercurial ulcer, that I shall be driven from the use of it with some difficulty. But I have observed so much accuracy of remark in Mr. Mathias's observations, that I shall certainly attend to them; being assured, that if such a state of chancre as I have alluded to, can be as equally soon cured as by the local use of mercury, that it certainly is desirable to omit it.

A case has lately occurred to me that does seem so strongly in opposition to the apparently well founded reasoning of Mr. Mathias, on the local application of mercury, that I do not willingly give it; but as I am sure that gentleman's only object is to do good, and not to establish certain opinions without they are founded on facts, I am induced briefly to relate it.

A person came to me who had been under the influence of mercury for seven months. He was very much reduced, he was pallid, with a weak pulse, little appetite, night sweats, and great pains in different parts. He had had chancres on the glans penis, which had healed, and ulcers had again broken out. He had drank hard during the whole of the time, and had been exposed at all hours. He had had, some weeks before I saw him, ulceration in the throat, which was considered venereal; this ulceration had returned several times.

During the whole of this time he had been more or less acted on by mercury, which had been given him in large doses, as on every fresh appearance of ulceration it had been considered to be venereal, and that the constitution had not been sufficiently saturated with mercury.

At the time when he came to me the penis was prodigiously swollen, and it was with great difficulty the prepuce was drawn off the glans penis so much as to enable me to discover a dark coloured sloughing ulcer almost all round the glans, which discharged a thin watery ichor. The penis, though so much swelled and inflamed, was not in the highest state of inflammation.

I directed him to abstain from mercury, and to wash the ointment from his thighs. He had bark and nitric acid, and was desired to keep the parts clean by injecting a decoction of poppy heads under the prepuce, and to foment the parts with it, and to keep a poultice over the penis made with that decoction.

He pursued this plan for four days, but the ulcer became worse. I, therefore, ordered the black wash to be injected very frequently under the prepuce, and some lint moistened with it, to be pushed down to the ulceration with a probe. This was assiduously attended to. In two days the ulceration was a little better, and by proceeding on this plan, in three weeks it became perfectly well. It must be observed, that he was taking the bark and nitric acid during the whole time; and, perhaps, the good effect which he

experienced may be supposed to have arisen from these medicines, and from his having abstained from mercury. Doubtless, all these must have had great influence, but I am disposed to think that the local disease could not have been so soon got rid of, if it could have been cured at all, by any other means.

Perhaps, it may be said that the bark and nitric acid, and the abstinence from mercury, cured this extensive sore, in opposition to the ill effects of the local application. I cannot think that I have so deceived myself,—certainly, I do not wish to deceive others.

There is one appearance that I have seen in this disease, that I believe Mr. Mathias has not noticed. Four cases have occurred to me, in patients who have been extremely reduced, where the whole pharynx has been covered with a whitish coloured coat, somewhat resembling a covering of curd, thinly spread over the whole surface. The tonsils and velum palati have been in each case palid and relaxed, and with a considerable degree of fulness, without inflammation, the fulness proceeding, apparently, from relaxation. There has not been any ulceration or excavation of the subjacent membrane, but the whole cavity, as far as can be seen by the person, strongly inspiring, and by that means elevating the uvula and velum palati, appeared covered by this substance. This exudation seemed to extend to the nose and larynx, as was evinced by a hoarse and nasal pronunciation, and form-

ed in the nose hard lumps, which were discharged in considerable quantities mixed with blood. As this appearance continued unchanged for several weeks, without any destruction of the subjacent parts, it could not be ulceration.

In a distressing case that I lately attended with a most excellent practitioner, I particularly intreated his attention to this appearance. I have not been fortunate enough to discover any remedy, either local or general, that has been of the smallest service. The four persons in whom I have seen it, all died. I rather suspect that it frequently occurs in this disease; but as the throat is generally full from relaxation, and being without redness or ulceration, and the patient being always weak and very irritable, it is not observed, as it cannot be seen without the velum be drawn up by a strong inspiration.

To record particular cases of this disease would only be to enter into the detail of human misery. I have never seen many cases of recovery, where the constitution has been very much reduced. The sarsaparilla having been so highly spoken of by all writers, I have not dared wholly to omit it. But if it be taken in the dose of a drachm at a time, the patient soon becomes disgusted with it, and without the dose be very large, no one pretends that it can do good.

The greatest benefit that I have ever seen from the effect of medicine, has been where cicuta has been joined with columbo and some of the bitters,

or with the bark; and, as I have said, from preparations of iron; where they can be so managed as not to excite heat.

The nitric acid seems likewise to be very serviceable, and as it can be given as a beverage, it does not disgust so much as many other medicines do which require to be long continued. I believe, however, that much depends on the energy of the constitution, and if that be seriously impaired, I am fearful that little is to be hoped.

I have lately been consulted in an unfortunate case, which had been supposed at first to be venereal, where the powers of life had been so exhausted, that, as it was impossible to recommend bleeding, the sarsaparilla and other remedies were fairly tried without effect. The surgeon who attended is a practitioner of great medical sagacity, and he had the aid of a physician of the most distinguished eminence in the country.

When I first saw the case I despaired of success, and explained my fears to the relations, desiring them to consult some of the first physicians and surgeons in London, which they did. The sarsaparilla was tried in all shapes, in such doses, that if it had possessed any active powers they must have been observed, as the case excited great interest, and every appearance was accurately watched. The result was unfortunate.

I only reiterate these unhappy failures, in the hope that something may be discovered to arrest the progress of a disease, which I fear is yet with-

out any thing like a certain remedy. I must again repeat, that nothing which I have said applies at all to the practice of bleeding, which Mr. Mathias recommends. I take for granted that what he has written on that subject is correct. No case has occurred in my practice, since I have seen his work, in which it could be recommended, on account of great debility.

*Some Observations on the Effect of Caustic Alkali
in Scrofula.*

A Pamphlet was published in 1811, by Mr. Brandish, surgeon, of Alcester, in this county, on the effects of caustic alkali in scrofula. I know that Mr. B.'s practice in that complaint has been very extensive and successful, beyond any thing that I have known or heard of in the practice of others.

Whilst that disease has been the opprobrium of almost all other medical men, physicians and surgeons, he was proceeding in the cure of numbers of unfortunate beings, who must otherwise have perished, or have dragged out a life of misery, diseased and disfigured by that loathsome complaint.

Soon after I came to this town, about thirteen years since, I was called to perform an operation upon a patient of Mr. Brandish's and Mr. Jones's, at Alcester, when, for the first time, through the polite attention of his partner, Mr. Jones, I saw some persons who had been under their care several years before, and were then well.

One of these was a middle-aged woman, who had had such disease in and about the wrist, that some of the carpal bones had been removed, or had been separated by the disease, and the bones of the arm were united to the remaining carpal

bones with considerable irregularity. The arm was, however, perfectly useful, as was evident enough, for the woman removed it from the washing tub, at which she was labouring, to enable me to examine it. Another case had been almost as bad an one as this. They had other marks about them of the disease having been very severe.

I believe that both of these women must have suffered amputation to have saved life, if they had not fallen under Mr. Brandish's care.

About the same time a very fine young lady, about eighteen years of age, was placed under my care for a female complaint, and by accident I learnt from her mother, that she had been under Mr. B.'s care for scrofulous affection of the knee, some years before. Her mother told me that she had been long under the care of the most celebrated surgeons in this and the neighbouring counties, one of whom I knew to be a truly scientific man, and of great celebrity, and that it was the uniform opinion of all of them that amputation was indispensably necessary to preserve her life. She carried her daughter to London, and had a consultation of three of the most celebrated men there, who were decisively of the same opinion.

In London she heard of some extraordinary cures which Mr. Brandish had performed, and happily for her daughter she was placed under his care.

The result was a complete cure. The knee

was not perfectly flexible, but it was sufficiently so to enable her to walk without much lameness.

These cases afforded such evidence, that although I have always entertained a very liberal share of scepticism about wonderful cases, and the effects of specifics in the cure of disease, to doubt would have been to have shut my eyes and my understanding against incontrovertible facts. Here was not a single isolated fact, not one which had been saved, Heaven knows how, but three decisive cases, in each of which the limb had been preserved, and which, in all human probability, could not have been saved by any other means.

I mentioned what I had seen to many medical men, but I found few that believed with me. I was not much known, and I suppose I was considered a credulous blockhead that would believe any thing. Since then I have seen many extraordinary recoveries, and I most heartily wish that the practice was more known, because I feel assured that it would be the means of saving much wretchedness.

I believe that much of the incredulity which I witnessed among the medical men with whom I conversed on the subject, arose from this circumstance; that Mr. Brandish, for some reason or other, thought proper to keep the remedy a secret for many years, and it was not surprising that regular men should withhold their credit and doubt the efficacy of a practice so closely approaching to empiricism.

Mr. Brandish states in his pamphlet, that he does not claim the merit of having discovered the efficacy of the caustic alkali in scrofula. It would have been strange indeed if he had made such a claim, as it is very well known at Alcester and in this neighborhood, how he became possessed of the knowledge of this remedy, which he kept secret for so many years.

But on this part of the subject I do not know that we have any right to put interrogatories, as it is now made public. This very circumstance has, however, I know, prejudiced many men so much against him, and unfortunately against the practice, that they have, absurdly in my opinion, refused to try the effect of the remedy, concluding that the whole is a story dressed up to deceive.

Mr. Brandish has likewise not weakened the prejudices which this secrecy has produced, by the manner of writing his pamphlet, which is full of petty details about making liniments and cerates, and how to make mercurial ointment, and particularly how the grand arcanum is to be made, with a great deal of other trifling about diet, &c. This has tended to strengthen the opinion that the whole is a stale empirical trick to delude the public.

I lament all this, as it has certainly been the cause of this powerful remedy being disregarded, which, if medical men can be induced fairly to try, must, I think, be generally adopted, being infinitely superior in its effects to all the other medicines that have been heretofore used

Mr. Brandish, in aid of the caustic alkali, has recommended that mercurial ointment should be rubbed in, and this, according to his account, is of essential service in every stage and state of the disease

It must be quite evident to all persons who have frequently seen the disease, that this indiscriminate use of mercury can scarcely be used with advantage in the very different states in which scrofula appears. Mr. Brandish, I believe, did use it very indiscriminately, and it is to me a strong argument in favour of the power of the caustic alkali, that this strange practice should not have prevented its beneficial effect.

I should not have ventured to observe on this practice, from mere reasoning, in opposition to the opinion of Mr. Brandish, for I know his practice has been very great and successful, if I did not positively know that mercury sometimes does much mischief; cases having occurred to me, where mercurial ulceration has been produced by it.

When inflammation is going on in scrofulous ulcers, mercury should not be persisted in, as it will generally do mischief. After the inflammatory stage is over, and the glands are torpid, and where inirritability prevails, as it often does to a great degree, the stimulus of mercurial frictions may be very usefully had recourse to. After the glands have become ulcerated and loose, and flabby, with layers of membranous-like slough, in that state too, mercurial frictions near the part are

serviceable. In some peculiar states of the constitution, where the digestive organs are affected, where there is flatulency and pain, and where there is general torpor, the use of mercury given internally, for a few days only, will generally do good. If the mercury be long persisted in, debility will inevitably be produced, and if it existed before the exhibition of mercury, it will be increased.

I can speak with some decision thus far, as I have carefully watched its effects in a variety of cases, and I think that in those cases where mercury does not produce its peculiar disease, which it positively often does, that it retards the curative powers of the caustic alkali.

A case which I have seen a short time since among many others, has so clearly demonstrated the injurious effects of the indiscriminate use of mercury in scrofula, that I shall relate the prominent points of it, particularly as the gentleman had been under the care of a surgeon of great and deserved reputation, and who, contrary to his own excellent sense, had been induced to use mercury merely from having heard of Mr. Brandish's plan, or from having read his book, and from the almost uniform success which was said to attend his practice.

The patient was a young gentleman who had had scrofula when a boy, and had been always, more or less, subject to glandular swellings. The glands of the neck and groin became diseased, and those of the groin ulcerated. From

all that I could learn, true scrofulous ulcer was formed. The liquor potassæ was given, and the mercurial ointment was rubbed in, as Mr. Brandish directs. The injurious effects of the latter was prevented shewing itself some time, from some preventive cause or other; I believe from the invigorating effects of the caustic alkali. At length, the almost certain effects of the long continued use of mercury displayed itself. The ulceration extended, burrowing in all directions; the edges of the ulcers became ragged, pain came on, profuse perspirations and all the usual train of symptoms which mark mercurial debility.

This gentleman's life would have been absolutely in danger if this plan had been continued. It became necessary to get rid of every atom of the poison that had produced these symptoms. The thighs were ordered to be washed with soap and water, bark and nitric acid were given, with cicuta. Happily, this change of system had the desired effect, and the young gentleman is well, as, after the mercurial disease had disappeared, the liquor potassæ was given to him.

The medical gentleman who attended with me is decisively of opinion, that the whole series of alarming symptoms were caused by the effects of the mercury, and he will never have recourse to it again indiscriminately.

It is surprising, that these effects of the general use of mercury have not more frequently taken place among Mr. Brandish's patients; and the non-appearance of them can only be accounted for by

supposing that the powerfully invigorating effects of the caustic alkali, joined to a generous diet, and the beneficial influence of pure air, had counteracted them. If he has not altered his plan, now that he practises in London, I should fear that the caustic alkali will lose its character.

The occasional use of mercury is of service, but it appears to me that it should not be carried so far as to affect the mouth, except in those idiosyncracies where a very minute quantity does so. In short, I believe it never should be given if there be active inflammation going on, nor in any case, internally, but where there is torpidity of the stomach or bowels; and when locally applied merely to stimulate the inert vessels of the diseased glands to more healthy action.

Of the decisively good effects of the caustic alkali alone I have had many proofs, where not a grain of mercury has been used in any shape, excepting when joined with scammony as a purgative. I have never seen it do injury in any of the various states of scrofula, neither when active inflammation was going on, nor when the parts and the constitution were torpid and inirritable.

Although it certainly stimulates, it does not excite heat. Strange as this may appear, it is a fact. It acts powerfully on the secretions from the kidneys, and sometimes on the skin. If it were to fail in exciting one or other of these secretions, it would, perhaps, do harm. But as far as my experience goes, it seems one of those inexplicable remedies that excites action without in-

creasing the temperature, and exhilarates, but unlike other stimuli, does not produce indirect debility.

Of the method of curé I dare not speculate. I restrain myself to speaking of its effects, as they have appeared to me, and leave to other more profound and philosophic men to investigate and unravel the mystic and abstruse cause of things.

Of this remedy I am almost fearful that I have said too much, and that my readers will think I have been hurried away more by my prejudices than by my judgment. But on a fair trial of its effects, compared with all other remedies which have been recommended at different periods, I believe it will be found the most useful. Certainly it is not a specific; it will not always cure; but it does seem to be intitled to the name of a deobstruent when used to remove glandular obstructions (if that old fashioned word be not quite obsolete) more so than any other medicine that has come under my observation.

In such a disease as scrofula, it cannot be expected that any remedy can rapidly cure. The caustic alkali requires to be used for a long time, and the dose in which it must be given, to have its proper beneficial effect, must be much larger than has been usual. Before Mr. Brandish made public his plan of treatment, it was not known what large doses might be given. This is an important circumstance to be attended to, and for a knowledge of it we are entirely indebted to that gentleman.

As I have no intention in this place of observing largely on the effects of the various other remedies which have been recommended in scrofula, I only solicit the attention of my readers to that of the caustic alkali. I must beg to observe that, with the high opinion which I entertain of its efficacy, I seldom rely on it alone, as I have no faith in specifics. Scrofula is so varied in its symptoms and its appearances, that remedies must be occasionally varied according to those symptoms.

In all cases, occasional brisk purging is decisively useful. Tonics are almost always useful, but more especially the preparations of iron. Mercury, as I have said, certainly sometimes does service, but it appears to me always to be injurious, if it be carried so far as to affect the mouth. When generally given, as recommended by Mr. Brandish, it is most pernicious, for, sooner or later, the mercurial disease will be produced. Without proceeding minutely into its effects, who, for instance, would think of using it in the hectic, which often attends extensive scrofulous ulceration, or when patients are weakened by long continued disease?

A pure and dry air seems to have a great effect on scrofula, as does warm clothing, and a generous diet. The disease being certainly frequently excited by mere debilitating cause, even in persons where there has been no apparent constitutional or other predisposing cause, it would, *a priori*, appear, that to invigorate and strengthen

the habit would be serviceable; and, on this principle I believe the whole plan of cure must depend, varying the means according to the peculiar state of the disease.

From the powerful effects of caustic alkali in this glandular disease, I have been induced to try it in some other complaints. Observing that it does not excite heat, I have ventured cautiously to try it in tubercular consumption, as it is well known how fatal that particular kind of phthisis is in some scrofulous families. From the mucous glands of the lungs being so much affected in that complaint, something might be hoped from its apparent deobstruent effects. There is, however some objection to its use in this disease, on account of its quickening the pulse, and of the well known injurious effects of increased circulation through the pulmonary system on inflamed lungs. Whether some combination of it with digitalis may not be serviceable, if it could be so managed as not to accelerate the circulation, is yet to be determined.

In that wasting disease, which so much resembles some kinds of phthisis, which is produced by disease, and enlargement, and ulceration of the mesenteric glands, I am disposed to think that it may be useful. I dare not hazard any opinions upon either of these subjects in the present state of my knowledge. On its effects in these complaints, and on some others, I shall perhaps, at some future period, communicate any observations that may occur, if any thing

should occur, which I consider worthy of submitting to public consideration.

I respectfully solicit the attention of the profession to these slight hints.

Under this name of scrofula it does not appear to me to be quite satisfactorily proved, that certain affections of the glands and lymphatics, and muscles, and ligaments, and bones, all of which have been called scrofula, do depend on the same cause. I have seen them existing independent of each other in the sanguine and phlegmatic. I have seen opposite parts, opposite in their habits and organization, affected with what all men agree in calling scrofula, but not at all resembling each other in any thing but in producing debility, being independent and with no mutual connexion.

I must not at present proceed farther in this investigation, but must content myself with saying, that I believe in many cases, where the constitution is not irremediably injured, that if we can persuade our patients to persevere, we have certain remedies, among the most efficient of which I class caustic alkali, that will, being properly given, with some degree of certainty, cure many of these loathsome diseases.

It does not come under my present plan to enter into the distinct treatment of scrofulous ulcers. The management of this disease, and of the varieties of it, would require more time than I can at present command.

On the Effects of Cinchona in acute Rheumatism.

THE practical work which Doctor Haygarth published some years since on this disease, has most unaccountably been almost neglected, at least, it has not made that general impression on the profession, which a work, supported by such a mass of evidence, might have been supposed to have produced.

Exclusive of the intrinsic, substantive merit of the book itself, I should have thought that a work bearing the name of Doctor Haygarth, so venerable from age, and most venerable for all that can dignify age, for learning, for science, and for benevolence, would have alone attracted the notice and respect of every man who practised medicine.

Most certainly, it has not influenced the practice in acute rheumatism, as far as my knowledge extends, so much as might have been expected.

I hope I shall be pardoned if I take the liberty of pointing out some practical observations, which I do humbly hope may be of some little service in rendering the practice, which has been recommended by Doctor Haygarth, still more effectual.

It happened, that the practice of giving the bark in the early stage of acute rheumatism was quite familiar to me, from having frequently witnessed

its efficacy, when I was a very young man; and the propriety of the tonic plan of treatment was strongly fixed on my recollection, by the reasoning of a learned and philosophic physician, whose lectures I attended.

More than thirty years since, I took some notes from cases for his clinical lectures, in which the depleting and lowering system was reprobated, and the contrary plan insisted on; and the good effects which I then witnessed have never been forgotten by me, but have influenced my practice ever since. That I have been beneficially influenced I cannot doubt, as in a multitude of cases during that whole space of time, my patients have generally recovered, and I have never seen but *one* case of metastasis in acute rheumatism, where that mode of treatment has been followed: on this I shall have occasion to make some remarks hereafter.

This disease having been called inflammatory rheumatism, or rheumatic fever, has frequently, and most importantly, I believe, influenced the opinions of medical men. As this disease in its first stage has all the exterior of high action, a flushed face, and heat, and a rapid full pulse, it requires much evidence to convince many men, particularly those who have learnt their opinions in a certain celebrated school of medicine, that bleeding and continued sudorifics are not necessary.

Hence the whole treatment of acute rheumatism is derived; hence it is, I believe, that men

have been bled, because it has been called inflammatory rheumatism.

The separation of coagulable lymph, forming a layer on the surface of blood after bleeding, has been long believed to be a certain criterion of morbid inflammatory action, and that, as long as it is observed, the bleeding may be repeated, not only with advantage, but that it is absolutely necessary to bleed to preserve life, so long as the buffy appearance, as it is technically called, appears.

Doctor Haygarth relates a case of a person to whom he had been called in, ill with acute rheumatism, who had been bled seven times, and the last time there was a buffy appearance on the blood, he says, "there was an inflammatory crust. It was manifest that such copious evacuations from his veins did not cure the rheumatic inflammation, but reduced the patient to extreme languor, debility, and even tears."

Thus it is that men frequently become the slaves of systems, and dare not break the magic circle in which they remain spell-bound, whilst reasoning and facts are presented to them in vain.

Where such a man as Doctor Haygarth failed, supported by a mass of evidence, presented in a tabular form, containing a long list of persons rapidly cured or relieved by this practice; when a man so celebrated, who does not claim the merit of originating the plan, but, true to justice and to science, has attributed to the illustrious Fothergill

the sole merit of the discovery, it is almost hopeless that any other man should succeed in rousing the attention of medical men to this most effective mode of practice.

I know that many persons, who are prejudiced against the use of the bark in acute rheumatism, suppose, that those cases in which it has been proved to have cured, when given in the earlier stages of the disease, must have arisen among the most debilitated inhabitants of large towns, among poor emaciated beings, who have been shut up in close, ill ventilated and filthy houses, ill fed, and over stimulated by the pernicious habit of drinking spirits. They suppose, among such a miserable race, where inflammatory action soon runs its course, and debility, with all its train of evils, comes on, that in such subjects the bark may be given with impunity.

They cannot believe that among a strong and athletic peasantry, well fed, and clothed, and lodged, and where the disease only occurs from persons being exposed to cold and wet, that the same treatment can possibly be used with propriety, or even with safety. They think among such a race of people that nothing can prove effectual but bleeding, and purging, and sudorifics. As a proof of the high inflammatory action, the buffy state of the blood is talked of (which, however, does not always take place in the highest state of acute rheumatism) and according to their theory of the disease, as long as this appears bleeding ought to be resorted to.

What I have quoted from Doctor Haygarth is a sufficient answer to this mode of arguing. It may likewise be necessary to remark, that the chief part of Doctor Haygarth's practice must have been in the country, as the city of Chester, in which he lived many years, is by no means a large place, and his rides were, as I have heard, very extensive.

The practice of medical men who reside in Birmingham is of a mixed nature, and enables me to judge of this argument with some degree of precision. We see enough of those diseases which arise from that irritable state which is produced from overexcitement and defective nourishment in some of the inhabitants of the town; whilst our practice in the neighbourhood, among a peasantry inhabiting a country, the surface of which is for many miles round a dry gravel, and the elevation being very great, almost as high as most parts of England, enables us to judge of the contrast between the diseases of these two very different kinds of men.

We know very well, that among these latter inflammatory action runs higher, and that bleeding is consequently much more necessary, and that they bear it much better than the inhabitants of the town. I can therefore form an opinion of this disease in the two extremes, for I have seen it repeatedly among both classes, and I can affirm, that it differs very little in its appearance and in its symptoms, and that the bark can be given with the most decisively good effect among those of

the most sanguine temperament, as well as among the weakened and palid.

After the long list which Dr. Haygarth has given, it would be in vain to relate cases. If this mode of treatment required that support, I could enumerate hundreds, and which, from the almost invariable rapidity of their recovery, would scarcely, I am afraid, be credited by persons who have not witnessed the effect of cinchona.

Doctor Haygarth recommends that the stomach and bowels should be opened by giving antimonials in such doses as will produce that effect. In this part of the treatment I have ventured to make some alteration.

I have always adhered to the principle of Doctor Haygarth's plan, by uniformly clearing the stomach and bowels by purgatives, but antimonials can seldom be so managed as to act on the bowels without producing vomiting, and the straining which the act of vomiting produces, excites such terrific pain in this most painful disease, where the slightest muscular action produces torture, that I have long since left off the use of antimonials, at least so far as to make use of them as evacuants. As sudorifics, after the bowels have been evacuated by any of the purgatives, I always use them so as to produce their full action if possible, as I consider moisture of the skin as of the very greatest importance previous to the exhibition of the bark.

The saline purgatives, which I usually gave some years since, I have now left off, as I thought that

they did not sufficiently evacuate the intestines of their contents, and therefore I have substituted for them calomel and jalap, or calomel and scammony. These latter cathartics seem likewise to act more powerfully on the system of the liver and biliary ducts.

Emetics do not seem to have such an effect in the early part of this disease as in the more purely febrile complaints, where they sometimes cut short the disease at the very outset, and before the return of the succeeding paroxysm. The act of vomiting would be most severely felt in acute rheumatism, where a finger can scarcely be moved without exciting the most exquisite pain. For both these reasons emetics seem to be at least useless.

General bleeding is well known to be a remedy which is much depended on. The excessive pain, the increased arterial action, the great heat, and flushings of the face, do certainly, a priori, point out the propriety of bleeding.

Whoever first dared to try the effects of tonics in the early stages of this disease, must have had great mental firmness. The great Doctor Fothergill first mentioned the beneficial effects of the cinchona to Dr. Haygarth, as that gentleman states, and I have learnt from other sources, that the idea originated with him. The debility which so rapidly follows bleeding in the inhabitants of such an immense city as London, and the prolonged diseases that succeeded, are said to have determined him to change his system. Many

of the physicians and other medical practitioners in London adopted his method.

This practice, however, made very slow progress in the country, and particularly in the northern parts of the kingdom, as it was supposed that there was something different in the disease, when it attacked the inhabitants breathing pure air, and consequently not obnoxious to the numerous causes producing debility which abound in large towns.

This, certainly, is an erroneous opinion, which I can take upon me to assert, as from the circumstance of my living in this large town, the population of which is between eighty and ninety thousand persons, and where the sources of debility are abundant, I have had many opportunities of contrasting it with the same disease when it has affected the peasantry in the surrounding country, and I can speak most decidedly to the fact, of the disease observing the same rules, and that the tonic plan is equally applicable to the inhabitants of the town and country.*

* *April 19, 1815*....I have at this time two persons who have been ill with acute rheumatism, the one a very delicate lady, inhabiting the lower part of this town; the other a young athletic man, 19 years of age, the servant of a farmer, in the parish of Harborne, about four miles from hence.

The lady was attacked with acute rheumatism on March 19. Her bowels were opened, and she had immediately after some antimonials with opium, which caused much perspiration. On the 20th she began the bark, and she was so well in four days, that I could not persuade her to take it any longer. The weather became extremely mild and warm, with the wind in the south. About the beginning of April the wind veered into the east, and the air became consequently very cold. She

So uniform has been the good effect of giving the bark largely and early in this disease in my own practice, that I have never bled one person

was exposed to the cold, and I was sent for on April 8, and found her with an inflamed throat, and a good deal of fever. She had an antimonial medicine, and a gargle with infusion of roses. On the 9th she was better, but complained of pain in her back. On Monday the 10th, I found her in exquisite torture. Acute rheumatism had returned, and the pains in the back and neck were violent. She was obliged to be carried up stairs by two men. As she had taken antimonials for the preceding disease, I immediately gave her the bark. She was better the next day, and on the succeeding day, Wednesday, the pain had left her. She complained only of soreness. Yesterday, April 18, she was so well that she became tired of her medicine; and as the wind is now in the north-east, and the weather very cold, I shall be surprised if she is not again attacked.

The farmer's servant had been ill three or four days before I saw him, which was on April 7. He was so ill, that as he thought he should die, he insisted on being removed from his master's comfortable house to his mother's, who lives in as wretched a cottage as can be conceived. This young man had been running after some cattle of his master's, and when he was extremely heated, he sat down in the field, and became chilled, as the weather was very cold, the wind being in the north-east.

In a few days he was attacked with this disease. When I saw him the pains were violent. He was stretched on some boards in the wretched kitchen of his mother's cottage, and the door facing the east, from which quarter the wind blew with violence, and there being a considerable vacancy between the door and the door-post, the young man was completely exposed to cold air.

I directed him to be put to bed, and to be kept there, as I have always considered it necessary to produce moisture on the skin, as a preliminary to giving the bark. He had an opening medicine, and an antimonial, with opium. These medicines had the proper effects. The bark was immediately given; and although this young man could not be prevailed upon to remain up stairs, but would go into the miserable kitchen, which was, as I have said, completely exposed to the east wind, and although the wind has blown from that quarter the whole of this week, he was yesterday, April 18, well.

These two cases are so completely contrasted, that I think they

who has been ill with it for many years, certainly not for the last fifteen.

I have witnessed the effects of bleeding in the practice of others, and I have seen such prolonged disease as the consequence, that there must something very extraordinary occur before I shall ever have recourse to it. When bleeding, and continued sudorifics, and warm bathing have been had recourse to, the disease is almost always extended for many weeks, and sometimes for many months, and if the attack comes on in autumn or winter, chronic rheumatism is almost the inevitable consequence, crippling the poor patient for the whole of the cold months, undermining the constitution, and rendering him subject to all the diseases which weakness induces.

Local bleeding does little in removing pain. When the tumefaction and pain have been defined, I have sometimes been induced to order leeches, but so little good results that I seldom recommend them, excepting in compliance with the wishes of the patients.

The same may be said of embrocations and fo-

must prove interesting and satisfactory, as certainly no two persons were ever more different in their habits and constitution.

April 25. The wind has continued constantly in the north-east, but neither of these persons have had any relapse. The young man has continued to take his medicines, but the lady has taken nothing since the 19th; fortunately, however, and most unexpectedly, she has not had any relapse.

N.B. The opium is only given in small doses, with an antimonial, so as more certainly to produce moisture on the skin, and not in such a quantity as to produce its anodyne effect.

mentations; when the swelling and pain are confined to a small space, which is not often the case, I have ordered them, as the patients and their friends almost always attach great importance to such applications. When there is scarcely one part of the body free from pain, these applications are quite out of the question. Where they can be applied they are only of temporary use, if they do afford any relief, which is not often the case.

Warm bathing is universally recommended, and certainly does sometimes give relief; but where the bark is given, there can be no intention of keeping the patient bathed in perspiration for a week or a fortnight together, or till there is the lateritious deposit in the urine, which is always anxiously looked for, and, therefore, it can only be recommended in some anomalous cases which occasionally present themselves. I have not seen a case that seemed to require this remedy for some months. A tepid bath does much service in relieving the soreness, and particularly the stiffness which remains in the muscles after the pain has abated. There is always much objection to the use of warm bathing during the violence of the symptoms, from the torture which motion produces; and although there is generally some temporary abatement of the symptoms during the time that the person is immersed in the water, it seldom gives ease for any length of time, not more than a few hours. Tepid bathing, in the first hours of the disease, if the

patient can bear to be moved, assists the action of sudorifics in moistening the skin, and producing an equable circulation; but if the diaphoretic medicine acts properly, the benefit that results from it is not worth the great exertion and pain it causes. If the person can be placed in a bath without much inconvenience, there can be no material objection to it.

Opium, and cicuta, and other medicines of that class, seem to have no effect on the disease, excepting as far as sometimes producing a short interval of ease. But they never can be depended on to produce even that effect, for as in all diseases where the arterial action is great, opium seldom produces ease, and it almost always increases the circulation, exciting heat and delirium. Cicuta, I have been told, may be given without producing these inconveniences. I have never seen it do any permanent good. Opium certainly does much mischief, if it prevents the bowels from properly acting, as it appears to me to be an essential part of the cure, that the bowels should be kept in a lax state.

The cinchona is the medicine which is to be relied on to cure acute rheumatism. It has generally entered into the plan of treatment, that after a certain undefined term, this medicine should be given. But before the hour has arrived, when it has been supposed that it could be safely administered, the patient has been suffered to remain in the most exquisite torture; no debility, nor weakness of the pulse, nor sinking of the consti-

tution, has been considered as authorising the exhibition of this medicine, without the urine has been observed to deposit what has been called a lateritious sediment.

I know that death has frequently happened without this appearance having taken place, and therefore the medical attendant has not thought himself justified in deviating from the beaten track. I can positively state, from what I have seen in a vast variety of cases, that this delay is quite unnecessary, and that, in general, there is no other preparation necessary than to purge briskly, to produce diaphoresis afterwards, and immediately, although the urine may be, as it most frequently is, of a high or brownish amber colour, that the bark may be given freely.

There is, however, one most important appearance of the urine, which I must beg leave most particularly to call my reader's attention to, as on it, I consider, depends the success of the case, if it should happen that the disease is not completely stopped soon after the bark has been given.

After the pain has ceased for one or two complete days, it often happens, from some circumstances, the rationale of which is not quite evident, that the urine begins to deposit, not the brown thick sediment which has been called *lateritious*, and which falls from the water when it is left at rest, but a perfect red pigment, almost of a carmine colour, and which stains the pot sometimes even of a deeper red than the sediment itself is. This may appear strange, and possibly

incorrect, but it is a fact which I have often noticed. Soon after this appearance, the patient becomes hot, and restless, and the pain almost uniformly returns.

I have seen this so often, that I can pretty certainly foretel the return of the pain, if immediate measures be not taken to prevent it. This appearance is not the consequence of a return of the disease, as it precedes it, because the disease, when in its extreminest violence in the first stage, is scarcely ever attended with any of this carmine deposit from the urine. What changes take place in the body previous to this appearance, I venture not to surmise; sufficient be it for me to point out the signal, which I believe should never be unnoticed, as a change of plan becomes necessary.

Upon observing this appearance in the urine, which I make a point of accurately examining in this and all other acute diseases, I always give a very active purgative. I am not content to give any of the saline medicines, but the scammony and calomel, and in such a dose as will act powerfully. After the purgative has acted, I give some antimonial, in such a quantity as to act on the skin; after which, and not before, the bark may be given again.

In some cases this appearance in the urine, with a return of heat and pain, recurs several times. I have not been able to account for this. It may be relied on as sometimes taking place, and that

active purgatives and antimonials will generally remove it.

I have seen this appearance of the urine hailed by some medical men, as being assured that it would prove critical, and that the bark might then be safely given. To me it is a sure proof that the pain will return, accompanied with great heat, and I have uniformly found the bark injurious. I, therefore, perceive this appearance with alarm, and immediately proceed to give purgatives; and, indeed, it is sometimes preceded by some costiveness, or if the bowels are open, the secretions from them are not in sufficient quantity.

I have sometimes seen this appearance return, often in cases which have been suffered to proceed for several days before medical attendance has been required; and many times when the patient has been kept in profuse perspirations. It certainly does appear when the disease has been seen soon after the attack, and where the bark has been given in the earliest stages. I attended a lady in the winter of 1813, where these attacks were repeated several times, at the interval of five or six days, and where every possible attention was paid to the state of the secretions from the bowels. Under the worst and most frequent of these recurrences the patient will, however, recover sooner in general than when bleeding and long continued sudorifics are used, and the weakness afterwards, as far as my observation goes, is never so great or of such long continuance.

Exclusive of the advantage which is gained by shortening the period of this very painful complaint, there are seldom any of the sequela which are observable, more or less, after acute rheumatism has been treated by bleeding and purging, and by the long continued use of sudorifics. If this complaint happens in the early winter months, it is well known that chronic rheumatism almost certainly torments the patient during the remainder of the cold weather, and guaiacum, and medicines of that class, are necessary to be taken frequently, as I have known with little, if any good effect, till the warm weather has decisively commenced. I have many times seen poor creatures dragging their swollen and painful limbs after them for many weeks after the acute disease has terminated, with sallow countenances, and rendered liable to fresh attacks upon every exposure to cold, from the irritable state to which they have been reduced, more by the very means which have been resorted to for their relief, than by the disease itself.

When the bark is given properly, these consequences seldom, if ever appear, as the disease is often completely cured in a few days; and I have often seen persons who have had the complaint with great severity, attending their business in ten or twelve days. In cold and moist weather this is very improper, as it exposes them to a recurrence, which sometimes may be more violent than the original attack. But I repeat, that I have many times witnessed this.

Superadded to the important advantages which I have enumerated, I must not omit to mention that metastasis, that most dangerous event seldom takes place. I have observed the disease innumerable times, and I have seen but one case where the bark was properly given, that that most alarming change in the seat of it has ever occurred. When the other plan of treatment is adopted, it certainly does take place too frequently. If no other advantage can be claimed for the one mode of treatment over the other than this, it would alone be sufficient to establish its reputation.

Of the various injuries that are said to attend the early administering of bark in acute rheumatism, I know nothing ; I have not seen any thing injurious, either at the time, or after the termination of the disease. I believe all that have been mentioned are ideal ; as it is absolutely impossible, if such injurious effects do take place, that I should not have heard of them, among the great variety of cases that I have seen. I am, therefore, quite certain that I have not embraced these opinions rashly, and I do urgently recommend this mode of treatment to every medical man who has not yet adopted it.

From the great number of cases that Doctor Haygarth has related, I did not think it necessary to bring forward others ; but as I have once been attacked by it with great severity, perhaps a relation of the symptoms, and a detail of the method which was adopted to remove the dis-

ease may not be uninteresting. This will indeed be “*renovare dolorem*.” I never shall forget the excruciating torture that I felt upon the slightest motion. The most trifling movement even of one of the toes, excited such exquisite agony, that if it had continued uninterruptedly half an hour, I should think that life could not have been sustained.

I am a strong muscular man, and my health has been almost uniformly good, excepting when I have been exposed to cold and moisture, and when I have been occasionally attacked by catarrh. Four years since I was exposed to rain, and was obliged to remain sometime in my wet cloaths. The second day after I was not well; I had those uncomfortable feelings, such as chillness and flushings of heat, which usually precede catarrh, attended with slight pains about my loins.

On the third morning I had an increase of the pains in my back, which occasionally shot down my thighs, but they were not so severe as to detain me at home, and as I wear flannel next my skin, I firmly depended on that to protect me from any serious illness. I went through the business of the day without difficulty, but at night I was suddenly, indeed almost instantaneously seized with excruciating pain in my back. Had I been transfixed by a javelin, I could not have been more instantly incapacitated from all motion.

I attempted to rise from my chair to get to bed, but it was impossible, as exclusive of the inde-

scribable pain which I felt, I had no power over the muscles of my thighs and back. I have a very vivid recollection of my sensations, and certainly never shall forget them, but language is inadequate to express them. I should think if a large ragged nail were driven with great force into the dorsal vertebræ, it would something resemble it.

By the assistance of several persons, I with the extremest difficulty was dragged up stairs, and was placed in bed, and when there, I lay extended on my back, without the power of moving a single muscle without producing torture equal to that of the rack.

I passed the night in this torture. Great heat came on, with thirst, and all the usual train of symptoms. I retained my senses, and was quite aware of what ought to be done. I knew very well that the first thing which was necessary towards relieving me, was to take a brisk purgative; although nothing was ever more impressed on my mind than this necessary preliminary towards a deliverance from the torment I was in, it was impossible for me to muster up mental resolution enough to take this. The fear of any motion deterred me. I therefore passed another day and night, in the vain and foolish hope that there would be some cessation of the disease.

Although I was convinced of the fallacy of this hope, I was so completely subdued by the fear of torture, that it required a most powerful exertion of intellectual firmness to conquer my alarms.

The medicine having acted copiously, I only

took a single dose of an antimonial preparation, and determined to begin with the bark. I should not have ventured to have had recourse to it so early with any of my patients, as I am always desirous to have the skin moist, if it be possible.

I took thirty grains of bark, and repeated it every three hours; this was continued for twenty-four hours. In that short period great relief was afforded. The pain was lessened, the heat was likewise diminished, and I slept some time. The dose of the bark was increased to forty grains every four hours, and I drank some port wine negus. The pain gradually abated, and in less than another twenty-four hours there was almost an entire cessation of it.

Three days after the bark was taken, there was scarcely any thing which could be called pain, after the previous suffering; the muscles were sore and stiff, but I could move without much inconvenience.

The water had been very high coloured, small in quantity, and without any sediment, till the bark had been taken three days. It then deposited a considerable lateritious sediment, but before this appearance was observed, all serious disease had disappeared. In fact I was, comparatively speaking, well. The pains were nothing, there was some heat, but my skin was moist, and my appetite was good.

A gentleman, who was kind enough to see some patients for me, saw me this day, and in eight days after met me walking in the street.

I believe that no man had ever a more severe attack of acute rheumatism than this was. I am quite certain, if I had been bled, and sweated, and blistered, and stewed in hot water, and had waited for the lateritious sediment to be deposited from the urine, before I had taken the bark, I should not have been out of my room for weeks, nor in my business for months.

I blessed the names of Fothergill and Haygarth; for by their admirable firmness in combating most powerful prejudices, and by their sagacity and science, I was relieved from the tortures of the rack; and hundreds have had and will have, if this plan be followed, cause, as well as myself, to beseech heaven in favour of these illustrious men.

I hope the statement of this case, which is precisely related without adding or subtracting any thing, may be read with the interest it deserves. It is a true description of what may be frequently expected to take place, if my readers can so far divest themselves of their preconceived opinions and prejudices, as fairly to try this medicine.

Let them recollect, that I have no purpose to serve in thus writing. I have no theory to support, and, therefore, have no reason to adapt a case to make it bend and support a favorite opinion. I lay claim to no discovery; I wish not to bolster up a name by insidiously applying to myself the discoveries of other men. It is to those great men, Fothergill and Haygarth, that

I render praise and thanks. Praise for general good, and thanks for individual benefit.

I do hope that the statement of this case will have the effect which it ought to have. I hope it will be believed and acted on, and then I shall receive all the reward that I can wish,—the gratification of knowing, that many miserable beings will be rapidly restored to ease, and to comfort, and to health.

Unfortunately, however, so rapid a termination of acute rheumatism cannot always be expected to take place. A variety of causes may occur which cannot be either foreseen or explained, which will sometimes produce a recurrence of the heat and pain, and sometimes the more alarming event of metastasis may take place, as is shewn in the following case.

A lady, being exposed to cold and damp, was attacked by this disease. I recommended that an aperient should be taken, and after the vessels of the skin had been relaxed by antimonials, she took the bark.

She began with ten grains, which were increased to fifteen every four hours. As she had great objection to the taste of the bark it was not taken very regularly. The pains were chiefly in her arms and hands, but not with the extremest violence. On the second day after the bark had been taken she became easier, but some pain continued, and in the night it returned.

On the third morning I was awaked early, and was told that the pains had suddenly abated, and

that she was quite easy. On visiting her, I found that the pains had ceased almost momentarily. Her intellect was obscured, indeed she was almost in a state of stupor; her pulse were very slow, not fifty in a minute; sometimes very low, and at intervals throbbing violently. It appeared as if the right ventricle were distended with blood, and that there was not sufficient energy to expel it into the aorta, but by a powerful effort.

Amidst this kind of stupor she was in her senses, but there appeared a most unusual apathy and indifference to all about her, which was diametrically opposite to her usual habit; and she almost unwillingly, as it appeared, answered any question that was put to her.

I learnt, with some difficulty, that she was absolutely free from pain, and that it had left her, as I had been informed, instantly; that she had a heaviness and oppressive feel about the heart, with some palpitation, and that she found a listlessness and inaptitude to speak or stir. She had not the least pain in the heart, or in any part about the chest. While these enquiries were making, which was done in haste, the pulse became more oppressed, with a convulsive throb at short intervals, as if the blood was impelled with the greatest difficulty.

I had never seen the disease which has been called rheumatism of the heart, but from the instant cessation of pain, and the peculiar sensation which was immediately felt about the heart, and the stupor that came over the intellectual facul-

ties at the same time, I could not doubt that metastasis had taken place; but as there was no pain about the heart itself, I could not suppose that it was that terrible disease.

To bleed in such a case of extreme debility would have been improper according to all that I had observed, and I had long left off being influenced by the name of a disease. As there was no time to be lost in long deliberation, I decided on giving the most diffusible stimuli, and hastily gave some æther, intending to follow it with ammonia.

The æther excited, as this lady has since told me, a very strange shock in every part of the body. It rapidly excited the action of the heart. I persuaded her, after a short time, to swallow a little more in some brandy and water. This had the most decisively good effect. The pulse became equable and stronger. The medicine was repeated at intervals during the day, as there was a tendency to a return of the lowness.

As the pulse regained its regularity, the oppression about the heart was removed, and the mental debility was in an equal degree relieved.

She had a return of the pain in her hands and arms on the next night. These were removed by returning to the bark, which was given on the succeeding day, and was taken with regularity. She had occasionally some oppressive feel about the heart for many days after, and a good deal of languor, which were always removed by taking some ammonia and spirit of lavender. This cor-

dial she took with tincture of bark for some time, and she gradually regained her former health.

This was the only case of metastasis that I have ever seen, where the cinchona has been given. I believe if this lady had been bled death would have been inevitable.

It does not appear quite certain whether the affection of the heart or the head was the cause of these alarming symptoms. It may be conjectured that the ventricles being overloaded by some particular detrimination of the blood to the heart, collapse of the vessels of the brain might have taken place, from the column of blood not being impelled to the head, producing mental debility; and that, by bleeding, the right ventricle would have been relieved from its overwhelming load, and thus its contractile action being restored, the blood would have been propelled to the brain, and its energy would have been re-excited. Whilst others may suppose, that by the action of some unknown cause, on the sudden cessation of the pain in the extremities, immediate torpor of the brain took place; thus impeding the nervous influence on the heart, producing inirritability of that most important organ, and consequently incapability of the ventricle propelling the column of blood into the aorta.

The latter appears to me to have been the more probable cause, as there was no pain about the heart, and the apathy and almost stupor appeared, as I learnt, instantaneously.

This lady has since told me, that she felt a

pleasing tranquillity, a complete indifference to all external things, and she believes, as I do, that she was almost in the very act of dying. In a person so tenderly affectionate and feelingly alive to her family as she is, the overwhelming interruption to all mental feeling must have been instant and complete, for she has assured me often, that no thought about them occurred. I cannot help thinking, therefore, that the cause of these appearances must have acted instantly on the sensorium, and that the affection of the heart was secondary.

Be this reasoning correct or not, I cannot help thinking that the diffusible stimuli were the only remedies that could have relieved this alarming attack, and this opinion was strengthened and confirmed by that most accurate observer and scientific physician, Doctor John Johnstone, who coincided with me in thinking so, and who visited this lady several times afterwards, and directed the ammonia to be given.

Although the early exhibition of the cinchona will, very frequently, as I have said, cut short the progress of the disease, cases will sometimes occur which are not explainable, where the heat, and swelling, and pain will recur at short intervals for many times.

Sometimes this recurrence may be traced to exposure to cold, and frequently I have thought that it might be attributed to the state of the bowels, and some connexion with irregularity in the system of the liver. It most frequently hap-

pens that this recurrence is preceded or attended, with the red deposition from the urine which I have noticed before, staining the pot with a pink colour, at other times with a dark red. I have so frequently seen this appearance of the urine in some affections of the liver, that I have been induced to suspect, that in this state of acute rheumatism there is some connexion with irregularity in the secretions of this viscus.

Is a pink or carmine deposition from the urine ever observed where the liver is not afflicted directly or remotely?

I have seen a cessation and return of pain sometimes observe almost regular periods, and when this is the case, I believe it scarcely ever recurs without the appearance of this red deposit. This supposed analogy has induced me to give uniformly brisk cathartics, to emulge, as it has been called, the vessels of the liver, and to give activity to its secreting system. This practice will be found decisively beneficial whatever may be thought of my theory, and I again repeat it, to excite the attention of my readers to what I deem an important circumstance, which is founded on such repeated practical proofs of its efficacy, that I hope it will not be overlooked.

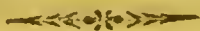
I had a remarkably obstinate case of this kind some months since, when the recurrence of pain was almost as regular as some quartans, and which was always preceded by cold shivering and a hot fit. The efficacy of brisk purging was clearly shewn in this case.

This case extended to more than a month, with intervals of ease of nearly four days. It was at length conquered without leaving chronic rheumatism or excessive debility. I tried the effects of emetics after the third recurrence of pain, and from the regularity of its period, I was induced to try the solution of arsenic, but without any beneficial result.

Some writers have conjectured that there is some analogy between acute rheumatism and ague. This almost periodical return of pain does something resemble the paroxysm of intermittents. In this neighbourhood there is so little of that disease, that I know not how to reconcile myself to that opinion. The surface of this neighbourhood being gravel, and elevated nearly 500 feet above the level of the Severn, I have never seen but two cases of intermittents since I have resided in this place.

I have thus slightly sketched what I have observed in the treatment of acute rheumatism. It is a plain narration of facts, and if it influences any one to have recourse to the same plan of managing this disease, I believe he will not be disappointed in his expectations. The reader may rely on its being a faithful statement. The prominent points are stated, minute parts are not mentioned, as much must, in all cases, be left to the discretion of the attendant practitioner, to vary or modify the plan according to the occurrence of anomalous symptoms.

APPENDIX.

*Case of Retention of Urine from enlarged Prostate.*

AFTER this work was in the press the following case occurred :

On Friday, March 24, 1815, I was sent for to a gentleman aged 73, who was unable to discharge any water from the bladder. Doctor Johnstone had seen him, and prescribed for him, and desired that a surgeon might be sent for to introduce the catheter, if possible.

This gentleman had not passed any urine from Wednesday night. The catheter was attempted to be introduced, but it was found impossible. A small instrument was then tried, and afterwards a catgut bougie, but in vain. As it has been mentioned by some writers that a large catheter will sometimes pass when a small one cannot be introduced, to leave nothing undone that had ever been recommended, I tried a larger.

This did not succeed, nor have I ever succeeded when I have made the attempt. Indeed, it appears to me to be merely a plausible idea, and not founded on practical observation, as I have never known it succeed in my own practice, or in that of others.

This gentleman was placed in a warm bath ; cold local applications were tried ; and as I most fortunately had the able assistance and advice of Doctors Edward and John Johnstone, it may be concluded that nothing was omitted which either learning or experience could suggest to relieve this gentleman.

On Saturday the bladder had become greatly distended, and could be felt high above the pubis. Not one drop of water passed. This is not often the case, as sometimes there is a few drops evacuated at intervals. I had ascertained, that the prostrate gland was very much enlarged, and I learnt from the patient that he had had great difficulty in making water for many years, and about six years before there had been a complete suppression for six or seven hours.

During the whole of this day, although there was such great distension of the bladder, our patient, who was a most placid, quiet man, never complained of much pain, even when he was very particularly asked that question, nor were his pulse much increased in quickness or fulness, nor was there much heat.

As three days and nights had elapsed without any urine passing, it was the opinion of the physicians and myself, that some operation must be had recourse to, but our patient would not consent to it. He passed another restless night, but he had some sleep ; and even on the Sunday morning he did not complain of much pain, and

although his pulse were quicker, it was not so much as might have been expected.

On Sunday morning he consented that the operation should be performed. Unfortunately, the catheter could not be introduced so far as the membranous part of the urethra, therefore I could not derive any assistance in my dissection to the prostate, by being guided to it by that instrument.

The gentleman was secured as for lithotomy. On the incision being made through the integuments, I tried to find the urethra, but I felt no disappointment when I could not discover the least vestige of it. I, therefore, proceeded dissecting onwards to the prostate. That it was much enlarged I knew, from my previous examination per anum, but in pushing the knife through it I was astonished to find the thickness of it.

Mr. Bangham, surgeon, who was so kind as to assist me in the operation, examined with his finger, and he thinks with me, that it was more than two inches in thickness.

A prodigious quantity of urine rushed out, more than is usual in diseases of this kind when they have been of so long continuance, as the bladder generally becomes thickened and of smaller capacity.

Our patient bore the operation with great firmness, and as there was little hemorrhagy, he was less weakened than could have been expected from his great age. The physicians prescribed

for him, and he continued tranquil and easy through the day.

He had copious evacuations in the night from some castor oil that had been ordered for him; the water drained from the wound as it was secreted into the bladder, and he had comfortable sleep; his pulse remained precisely in the same state as before the operation.

On the Monday morning he appeared as well as it was possible for him, under all the circumstances of the case, excepting that the belly still continued very hard, and full, and sore.

On Monday afternoon the belly continued hard and full, his pulse much the same as in the morning, and he was composed and easy. At night his pulse were very low, and he was chill. He had a little wine, which excited arterial action, and his pulse became better.

On Tuesday morning his pulse were much sunk, as were his features. He had some more wine, which roused him a little, and a medicine was ordered for him by Doctor J. Johnstone, with æther. This had some effect, as his pulse became better, and the countenance resumed its usual appearance. His belly continued full and hard. The water continued to drain through the wound.

On Tuesday night delirium came on and restlessness, which continued on Wednesday, although the pulse recovered their strength and firmness. The belly still was hard and full.

He continued getting worse till Saturday, when he died.

This was certainly an unfavourable case from the beginning. His great age, the extensive disease of the prostate, and the time that was suffered to elapse before the operation was consented to, were all against the success of it.

It was proposed to him on the Saturday, and he was persuaded by his friends, and particularly by his brother, who most anxiously and tenderly urged to him the inevitable consequence of his refusal; but he could not summon resolution enough.

In this case the puncture through the rectum was quite out of the question, as no finger could have reached beyond the prostate (so immensely was it enlarged) to have guided the curved trocar into the bladder.

To have cut above the pubis would have been likewise useless, as, in addition to the great disease of the prostate, the membranous part of the urethra was almost obliterated, and therefore there could not have been any hope of the urine being discharged through that canal again.

The little pain that this gentleman felt was surprising, for although the abdomen was so distended, and as hard as a board, he complained little, nor were his pulse scarcely affected. The fulness and hardness of the belly never changed.

THE following interesting case of hernia has occurred to me, and I have just time to insert it in this work :—

Mrs. —, of Moor-street, in this town, sent for me on Saturday the 22d of April last. She told me that she had been seized on Thursday evening with great pain in her bowels; that she had been subject to such pains, which were generally relieved by taking some opening medicine, and that she had taken castor oil and various other kinds of aperients, but without effect. That sickness came on soon after she was taken ill, and which had continued ever since. The abdomen was tense and tender, and was very painful. Her pulse were quick and weak.

I enquired if she had any swelling in any part about the abdomen, or upper part of the thigh, and I found that there was a small tumour on that part, which had been there a long time, she could not tell how long, nor could she inform me how it came.

I examined it, and found a small tumour somewhat like an enlarged gland, slightly moveable. I tried to move it from its situation in every direction, but found it impossible. As my patient is very thin, I traced it down to the crural ligament, and I thought I could perceive a small piece of strangulated intestine under the lower part of it, but I was not certain of this.

From the history of the tumour, I concluded that what I felt was a small portion of omentum which

had been in this situation some time, and on re-examining, I became more convinced that there was some intestine beneath it, but I could not absolutely ascertain the fact. All the symptoms were those of strangulated hernia.

The tumour was attempted to be replaced. This attempt was made at intervals several times in the evening, for the symptoms were urgent, and the sickness was unceasing. The infusion of tobacco was injected into the rectum, she was placed in the warm bath, and was bled. The tobacco glyster affected her very much; and while she was under its influence, I again attempted to reduce the tumour, but without effect.

The symptoms were so clearly and decisively those of strangulated intestine, that although the feel of the swelling did by no means resemble it, I recollected what that celebrated surgeon, Mr. Astley Cooper, mentions in his most valuable work on hernia.

In his second volume on hernia, chap. 4, he says, "these tumours are so small, that it is not without hesitation that the surgeon determines with respect to the operation; for they are, when small, generally covered with an absorbent gland, and with so much difficulty distinguished, that I confess I have more than once began the operation with much doubt about the nature of the tumour, making it rather the means of determining with certainty, than being assured that it was the disease which I suspected."

Assured from all these circumstances, and having my opinion strengthened by this authority, that it was necessary to proceed to examine the parts, I intreated her to allow me to do so. Nothing could induce her to consent, and she passed the night in great pain. I reiterated my intreaties on Sunday, but without persuading her to consent, and as I was fearful that I should have to witness what I have unfortunately more than once seen from the same cause, a lingering, miserable death, I requested she would consult some other person.

She saw Doctor John Johnstone, who stated the urgent necessity there was for the operation to be performed, and at length she consented, and it was done about four o'clock on Sunday evening.

On dissecting through the integuments and fascia, and the thin hernial sac, so thin, that although this sac had been protruded some years, it was not the least altered from its peritoneal appearance, a complete glandular-like tumour presented itself. This glandular-like appearance had likewise a glandular firmness, and I am quite certain, if such a case had happened to me early in my practice, and if I had not had some person by me that could have informed me better, I should have taken for granted that I had made a mistake, so entirely did it resemble an absorbent gland.

I divided the sac to the bottom, and then, on

turning this piece of omentum up, I discovered a small portion of intestine under it, and adhering to it. The adhesions were easily separated. I had some doubt as to the propriety of returning the omentum into the abdomen, on account of its firmness, as I well recollect what that great surgeon, Mr. Hey, tells us, that in a case where he had returned some omentum, he found it, on examining the body after death, like a thick rope.

As the omentum in this case was small in quantity, as it was very slightly discoloured, and as we know, that sometimes parts which have been long subjected to pressure soon recover themselves when that pressure is removed, I thought, on the whole, it would be most prudent to replace it in the abdomen, which was done.

On making an incision through the stricture, the intestine and omentum were still irreducible, and I was obliged to make an incision, as Mr. Cooper recommends, at the posterior edge of the crural arch, before I could reduce them.

This hernia had the most complete resemblance to a gland before the integuments were opened, and afterwards, of any that I have ever seen or read of. My patient being a very thin and delicate woman, I was enabled, before the integuments were opened, to trace the strangulated omentum down to the crural ligament. If she had been very fat it could not have been so clearly ascertained. From her thinness likewise, the external fascia was imperceptible, and the

sac, although the hernia had been down some years, was as thin as the peritoneal lining of the abdomen.

It is well known that the sac of femoral hernia is generally thin, seldom becoming thickened, as in inguinal hernia, but this was more than usually thin.

My patient had evacuations, from some castor oil that was given her, in a few hours afterwards, and before morning they were copious in quantity. Her sickness left her, and she had some sleep.

Doctor John Johnstone directed opening medicine for her the next morning, which she took, and has continued to take, so as to keep her bowels in a laxative state. The pain and tenseness of the abdomen subsided, she had comfortable sleep, and she is now, May 6, absolutely well, as the edges of the incision are united, and nothing, excepting weakness, remains.

Since my patient has been recovering, I have learnt from her, that the small tumour, which she had observed for several years, had never receded, but as it seldom gave her pain she paid little attention to it. She had been long subject to pains in the bowels, and she recollected that the tumour was then always uneasy.

On the Thursday evening, when she was taken so alarmingly ill, she told me that she had met with an occurrence that hurried her very much, and that she felt great pain in the tumour soon

after, but that it was not increased in size. I suppose the small piece of intestine was then protruded.

This case was, on the whole, a very interesting and instructive one, and I desired my son to infix it in his mind. It distinctly shew s the necessity there is of not relying merely on the feeling of the parts when the symptoms of strangulated hernia are present; nor even always on the appearance of those parts when the investing coverings are opened. For this case shews the necessity there is of not being satisfied with a very slight investigation, but that, if a gland-like substance should ever present itself, it should be traced to its very origin.

The thinness likewise of the parietes of this hernia was worthy of notice. From my patient being very delicate and thin, the external fascia was absolutely imperceptible; nor was the fascia propria to be discerned; and the sac itself was mere peritoneum; so that a very slight incision, if not made with the greatest caution, would have at once exposed the abdominal contents.

Hence the possibility there was of this piece of omentum (so peculiarly pressed and arranged as it was) being mistaken for a gland; as if a single incision had brought before the eyes of the operator this gland-like substance, he might have concluded that not having seen any fascia, nor those regular tunics, which writers so particularly describe, that he had only divided the skin, and that



he had before him an absorbent gland; and therefore, without proceeding to a farther and more minute investigation, might have left his patient to inevitable death.

Nor is it unworthy of remark, that a second incision was necessary to open the posterior part of the crural arch, before the intestine and omentum could be reduced.



FINIS.





